THE

BOSTON MEDICAL AND SURGICAL JOURNAL.

VOL. XC.] THURSDAY, MAY 7, 1874. [No. 19.

Original Communications.

THE MUTUAL RELATIONS OF DRUGGISTS AND PHYSICIANS.*

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IT was not till about seventy-two hours ago, that I understood that my province for this evening was to be so extensive as an address to others than the graduates of this School. Without further explanation, then, I shall try to mentally resolve this entire audience into a class who have received their diplomas, and shall trust that no one of you will be offended by the experience which you may hear or by the advice which you may receive.

After giving to these gentlemen the diploma with the seal of the Massachusetts College of Pharmacy, which sets forth their qualifications, and to which each one of them is entitled, the President of the College has invited me to say a few words. I understand that the pulpit and the bar have both in turn been called upon to speak to their predecessors; and, considering the position of the representatives of those professions who have undertaken the task, perhaps I ought to feel flattered by the invitation.

But, gentlemen, upon what shall I speak to you? I was not brought up to your profession, for by that name I choose to call it, hoping that you may, as you have the opportunity, elevate yourselves above the mere dealers in drugs and medicines, candies and fancy goods. There are matters, indeed, which may be, which ought to be, of interest to all of your profession and mine, and to the public at large as well, concerning which a few words, at this time, may not be inappropriate.

It is now some thirty years since I began the practice of medicine. About that time, I remember seeing an advertisement in a Boston newspaper:—"For sale. An Apothecary's stand and fixtures." And, among the inducements to purchase, was the statement that "it requires no previous acquaintance with the business." This was in the city of Boston, where we should find people boasting that things were done more by rule than elsewhere in the country. Thirty years ago, it would have been considered a difficult matter to find a carpenter, a cooper, a shoemaker, or any other mechanic in charge of a piece of work in his own trade, who had not passed through the regular stages of an apprenticeship for seven years, and journeyman. No man

* An Address before the Graduating Class of the Massachusetts College of Pharmacy, April 22, 1874.

would have been looked upon as a safe legal adviser who had not been trained in the whole mystery of the law; and he who had not at the least read his Bible, would have been considered a blind leader of the blind, had he undertaken to instruct us or our fathers in the mysteries of theology, or to explain the doctrines of sin and salvation, erroneous as the notions of either speaker or hearer might have been.

At the same time, he or she who professed to be born with curative powers, was sure of having a limited number of followers. In former days, a certain amount of mystery was supposed to be necessary in the treatment of disease. Education was not always considered a necessity; that is to say, what we call education to-day. Certain men were supposed, by the world at large, to have been born with the power to heal diseases, and others to have acquired that power by their prayers. Indeed, this belief has by no means departed; and there are enough of the weak-minded still left to encourage the hypocrite and blasphemer who prays for a living, and who advertises to the world that he is about to pray. Such weakness is more common in country communities, although in this city the medical practitioner not infrequently falls in with those, who believe that the seventh son of a seventh son has miraculous powers. There are still those who believe in incantations, and night after night, and week after week, the believers in spiritual manifestations consult the medium, whose whole work consists in prescribing the last new remedy for consumption, or in repeating the age of the grandmother, or the number of dead brothers, which the applicant has forgotten that he told her only ten minutes before.

Within a few years, another form of quackery has come into being, believed in by many, though not in any degree by those who practise it. You understand, of course, that I speak of that class of practitioners who pretend to believe that the more you reduce the strength of a medicine, the more you increase and develope its powers; and that a millionth of a grain has more power than a grain. The men who pretend to act upon this system—I do not know of one who does more than pretend—are aided in their sham by the progress made by chemists in analyzing drugs and discovering ultimate principles, which these fellows use under names which do not belong to them. The ignorant public does not and cannot know that the two per cent. of quinia extracted from "Jesuit's bark," or the ten per cent. of morphia extracted from the opium, is not an evidence of the progress of a so-called school of medical practice, which is not followed, even by those who profess it.

But there is progress; progress in your profession, progress in mine. And that progress will be more marked in the next fifty years than it has been in the last fifty. When it was first proposed that druggists should employ only those as assistants who were educated, or to be in part educated, outside of the particular shop in which they were employed, the proposition was laughed at by a large number of those who now believe it to be the true course. But how stands the record? Not a druggist, or pharmaceutist if you like the word better, of really good standing, but is anxious that his boys should have a better education than he had, and that they should require their successors to be still better educated. And if you, young gentlemen, are in earnest in acquiring knowledge, and determined to follow properly the course

which has been marked out for you here, and which really you have only begun upon, the day is not far distant when every drug store and its laboratory will be a successful field of scientific business, as some few now are; and the fancy goods store combined with the liquor saloon, as some few have been in years past, will take a lower position or be blotted out.

The same incredulity about its success was manifested when the Medical Department of Harvard University proposed to do away with the peculiar style of instruction, which had always been followed there and in all other schools about the country. The substitution of a more thorough course of medical education was pronounced impossible; students would not attend upon it; and this more surely because a verbal examination of ninety minutes at the end of the course was to be displaced by a written examination at the end of each year, and which might be protracted to twenty-seven hours in all. Those of us who were determined to succeed, and have succeeded to a certain extent, are in full accord with the gentlemen who stand at the head of your college.

It seems impossible that men were admitted to the degree of Doctor of Medicine, all over this country, with a single examination in nine branches, allowing ten minutes to each examination. That, however, was the fact. And we were warned that if we made the change proposed, the Medical School of Harvard University was destined to die, and its days were already numbered. But there were those who believed that an attempt should be made at progress, and that if death was to be the result, why, it could not be in a better cause.

What has been the result? The examinations have been spread over the three years. Twenty-seven hours have taken the place of the ninety minutes. The number of our students, in the four months' course of 1870–71, was three hundred and one. In the nine months of the following year this number was greatly reduced. Fifty-four only had adopted our new plan. In the year 1872–73 this number increased to ninety-three, and in the first term of this year we could count one hundred and eighteen students who were willing to undergo the severe examinations required for a degree. But how about expenses and income? for the former must have largely increased and the latter must have diminished. Why, gentlemen, the income of this year, which has but little more than *half* gone, has already been nearly a third more than it was in the whole of last year.

What has become of the other students? Why, they have preferred to go where examinations are easier; where the time is shorter; where the drill is less severe. We are satisfied that the change in the manner of study will be a success. The course which we have marked out is in parallel with the course which has been marked out for you, in taking you from the counter of the drug store and giving you extra work and study in the college laboratory. We have more branches of study, and our course is longer and harder. Let each and all of us do what lies in our power to make both colleges successful. The quack pretender, perhaps, can make money more rapidly and more abundantly than the educated pharmaceutist or physician. If pecuniary compensation is all that a man seeks, it is true that he can acquire it more easily in quackery and humbuggery than in fair and open work. That is a matter of taste. A thief or a forger can conquer

both in that game. That is a matter of taste also. I am aware that, in your profession, there are obstacles to be overcome which many others have not to contend with. It is true that the laws of the land have put taxes upon your sales perfectly unwarranted, and that many of these are so worded that even the most honest man cannot act under them without risk of punishment and disgrace. People complain of the cost of the medicines which you furnish, and yet do nothing in any way to aid you in getting rid of burdensome percentages laid upon the same article, over and over again. A stir in the moiety business within a few weeks past is, I trust, the beginning of a reformation by which those government sneak thieves will be got rid of, who spend their time in trying to trap honest pharmaceutists in breaking a law which, to appearance, was blindly worded purposely.

The duty of educating a doctor of medicine is not completed when he has pocketed his diploma. Indeed, he has then only learned how to learn. He may answer correctly all questions, but his practical education is to begin at that point, and will only be finished with his life or on his retirement from the profession. The duty of this college is to produce such a class of educated graduates as will be able to manufacture and inspect drugs for themselves. But your duty, gentlemen, was not completed when you passed the examination which entitled you to your diplomas. You may say that your course of study is practical throughout. No more so than ours. The older you grow in your work, the better it should be in every way, and science should go hand in hand with profit.

"The experienced doctor," so called, is, in many places, one who has grown gray in the practice of his profession without ever learning a fact which he did not read as an undergraduated student. And he who is often considered by the ignorant public to be the reliable druggist is no better. The object of both our schools is to teach the beginner how to teach himself afterwards.

There are relations between our professions which it is worth while to call attention to. It has been the case, sometimes, that the pharmaceutist and the physician have run in opposition to each other. This should not be. Every man has his preference for individual members of any trade and of any profession with which he has dealings. But you have no right to turn my patients into the hands of any other medical man who chooses to send his prescriptions to you. I have no right to require my patients to buy drugs at any other store than that which they are accustoned to deal at because he who keeps it is one who patronizes me. The physician who keeps a private remedy at a particular shop, which can be put up only on his order, and which no other druggist can understand, is a quack who has violated his word of honor. If you furnish drugs which I know to be inferior, because they cost you less than drugs of the first quality, that alters the case. Meanness is criminal wherever it exists, and should be punished.

Let us have a case or two from either side of the sheet. A medical man wrote a prescription for some morphia in pills. The size of the pills was excessive, and nothing upon the prescription indicated that there was any unusual call for what to most people would have been a fatally poisonous dose. There were no written directions, as there should be upon every prescription. The druggist very kindly said to the bearer of the prescription that he would prepare the medicine and

send it to the house. He tried for several hours to find the writer, and at last caught him at the dinner table, was kept waiting for a time, and was finally informed by the doctor that he never made mistakes, and that the prescription meant what it said. Of course, the druggist returned to his shop, and while engaged in preparing the pills, a messenger from the same patient came with another prescription. It seems that, without waiting to see his office patients, the doctor, on finding that he had made a blunder, too proud to acknowledge it and thank his informer for saving his patient, had hurried to the patient's bedside, changed his prescription as the result of new thought, received the thanks of the family for his attention, and repaid the apothecary with meanness for an act of kindness which prevented a coroner's jury.

I was knowing to the fact that a physician once advised some medicine to be thrown away because it did not come from A's shop, at which he traded, and no one in Boston knew so well as A how to prepare it. It had been purchased at B's shop. I also knew that B made all of that particular preparation which A sold. The bottle was refilled from the same fountain and pronounced to be all right. The first was a case of meanness. Meanness and ignorance combined formed the second, and both of them were deserving of punishment. I have known the same physician to change the form of a prescription, given on emergency by another who had been called in his place, simply that none but himself should have the credit of giving relief.

I care nothing who the physician may be, nor who the druggist. The physician has no right to interfere, except upon stronger ground than mere acquaintance or personal preference, or even family connection. Least of all has he the right to decry the druggist's medicine, unless he has good reason to believe it to be of inferior quality. You know, and the public should know, how much medicines vary in quality, and that the price and quality go together. It will be no credit to any of you if you sell medicine at retail for less money than your neighbor buys it for at wholesale. Take some of the common articles, which families keep on hand, and see the difference in cost: sugar of lead from fifteen to fifty cents a pound; sweet spirits of nitre from twenty-five to seventy cents; powdered rhubarb from twenty cents to two dollars; Hoffmann's anodyne from thirty cents to one dollar and seventy-five cents; arrowroot from ten to fifty cents; and other articles used solely in prescriptions at equally varying prices.

Another picture of the way in which meanness can be shown, and in which its dangers might be shown, is in the substitution of drugs. There are shops at which you can find anything you may ask for, even if it is not in the shop. Some years ago, having just risen from a sick bed, being taken with vertigo in the street, I stepped into a drug store, where I was not known, and took a seat. Not wishing to intrude without making payment, I asked for a chemical preparation; was told that they had it, and, after a long hunting about, a package was made up for me and paid for. When I got home, I tested it, and found it to be an entirely different article from that asked for, and sent it back, with my compliments to the one who put it up. I never had any notice of its having been received. A short time after this, I wrote a prescription for four pills, which were made and taken. It was thought best to renew them, by the number on the box. The

messenger, by mistake, took the box to that same shop. Eight pills were put up in place of four, and an active cathartic would have been taken in place of a narcotic, if the patient had not seen the box and its contents before the pill was put in some jelly for him to take.

Twice within a few weeks, I have had sugar-coated quinine pills, made by machinery, sent to patients in place of pills of quinine and hyoscyamus, as directed.

It is the duty of the physician to write his prescription plainly, and to put his directions for use upon the paper. It is the druggist's duty to put up the medicine precisely as directed, or not to put it up at all. If there be anything directed which he has not, the druggist has no right to substitute anything else without consultation with the prescriber; and this, whether the articles be inert or active, whether they be costly or cheap. You have no right to think that a sulphate or an acetate will answer the purpose as well as a muriate. You have no right to add more acid to a mixture than is directed, even if it be necessary to make a perfect solution; and even in so slight a matter as an excipient, you violate your duty if you make the simple substitution of one dry powder for another, or one mucilage for another. If the mixture be vile in its appearance, absurd in its composition, a liquid when the ignorant prescriber thought it would be dry, that is a blunder which it is not your business to correct. You might, in courtesy, notify him of the fact, but you have no right to change the mixture. You do not know, and cannot know, why a particular article is prescribed. This peculiar fault, peculiar impudence, if you please, is so common at one very popular drug store in this city, that for a couple of years past I have invariably cautioned patients not to have my prescriptions put up there.

Neither has the druggist the right to suggest to the purchaser that the dose prescribed is too large or too small. Within a very short time, a timid, nervous young lady, suffering from hæmorrhage from the lungs, and being miles from her physician, who had sent the prescription, accompanied with written directions for its use and his name appended to it, was frightened into long and dangerous neglect of herself by a druggist, who informed her that fluid extract of ergot, the best article known to-day for the prevention of hæmorrhage, is a slow poison and dangerous to use.

On the other hand, I contend that physicians have no right to prescribe unusually large doses of so-called poisons without intimating, in some way, upon the prescription that they are aware of what is written. Should I write for half a grain, or larger doses of morphia, I consider it my duty to write upon the prescription that "I have read over this prescription, and it is written as intended." Should I write for a medicine not often used, and about which there might be a mistake made, it is my duty to write down its other name also, if it has one, that the druggist may be sure of the article written for. Chloride of mercury should never be abbreviated, so that its identity with chloral hydrate may be questioned; and croton chloral should be so manifest upon the prescription that croton oil should not be substituted for it, when repose is intended as its effect.

With the best of care, every man is liable to make mistakes. Loss of sleep, or an attack of indigestion, either of these may make you or me commit an error. Under such circumstances, I have found it my

duty and pleasure to thank the druggist's boy for bringing me a prescription to be completed.

The wisest course for me is to keep a copy of every prescription that I write. Copying it makes me more likely to see an error. The proper course for you is to read over the prescription, and if it is to be copied, copy it then, before it is put up. The plan of delivering up the original to him who presents it is a bad plan. In case of doubt as to the author of an error, whether it be the writer or the compounder, the evidence of the original prescription should be in your hands for your protection. He who dispenses should have the writer's authority at hand for easy reference.

There is one matter of interest to all of us, and to the public as well, which I am not sorry to have an opportunity to allude to. I refer to the habit which some druggists have of prescribing. "Well," say you, "if a man calls on me to give a dose of medicine for a diarrhœa, or constipation, or cough, or sore throat in a child, why should I not give it to him? Why is not my prescription as good as that of any old nurse, or any other neighbor who has strayed in?" It is exactly as good, and no better. The sore throat, of which you know nothing, may be the result of insufficient dress, or of almond candy. The diarrhœa may be the beginning of a typhoid fever, or from want of flannel drawers. The constipation may be owing to too little food, or approaching disease of the brain. All of them may be simply symptoms of dyspepsia. One dyspepsia may require an acid, another an alkali; one needs a dose of physic, another requires food; one wants out-of-door air, and another a warm bed. One needs to have the patient stop his head work, and another would be benefitted by any change from his present style of life. Cough may have its cause in the head, in the throat, in the lungs, in the stomach. The very medicine which you give, without knowledge of the particular patient, and how to examine him, may be the means of aggravating the disease.

To say that this medicine is very simple, and, if it does no good, cannot do harm, is one of the most common of mistakes. If no other harm is done by it, there may be serious loss of time. But what is a simple remedy? You, perhaps, will say "tolu, ipecac, castor-oil, salts, rhubarb." Neither of these is so simple as morphia, or strychnia, or Prussic acid. The simplicity of an article or of its combinations has nothing to do with the usefulness or the hurtfulness of a medicine. If a man asks you for a compound cathartic pill, or for a dose of paregoric, that is all very well. I know of no reason for refusing him. If he asks you what the best cathartic for him is, or what would do his cough the most good, that is a very different matter. You know, and can know, nothing about it. If one man's meat is another's poison, and this is literally true, the parallel comparison will hold equally good concerning medicines.

The effect of the same medicine upon different individuals is very remarkable. And this cannot be wondered at by those who have seen the effect of different articles of food. Some of us know that so pleasant a fruit as the strawberry will produce difficult respiration in some who eat it. The clam and the oyster have similar effects upon others; and I have known the eating of poultry to do the same. Even the passing near enough to inhale the odor from a wheelbarrow of lobsters, I have seen followed by asthmatic breathing, and an eruption which

could not be distinguished from erysipelas. So an opiate stimulates one and depresses another.

I have a case on my books, which is a fair but ludicrous illustration of the value of such prescriptions as are "simple, and can't do any harm." I was called to see a little girl, who had been kept at school on Saturday till after her usual dinner hour. When she got home, vexed and tired, notwithstanding she had a slight headache, a slice of beefsteak would have relieved her. The headache led the mother to give a dose of castor-oil, and to offer gruel, which was refused. Sunday, no improvement, and tincture of rhubarb was taken. On Monday, no better; senna and salts. Tuesday, no better, and a dose of elixir pro. On Wednesday afternoon, I was called in and informed by Mrs. —— that, having no other medicine in the house, she had given the little girl an emetic, and it had brought up a great deal of bile, and she feared the child would never recover. The advice given was to feed the child, and let medicine alone. Yet these were all what are called simple medicines. With the exception of great weakness and loss of flesh, the child was in her usual health in twenty-four hours.

This case is a fair illustration of the value of prescriptions made at random, and, although you might not be foolish enough to go through so long a list, why should you not? If not competent to examine the patient, you are not competent to prescribe for him; and if it would be improper to continue prescribing ignorantly, so it would be improper to begin. Remember that many diseases have common symptoms at times, and it is only after days of suffering, in some cases, that the real nature of the disease is declared. The medicine which might be of service in a pleurisy or pneumonia, would be absolutely injurious to an overloaded stomach; still, the case is by no means uncommon in which chest pain, or irregular action of the heart originates in indigestion. The delirium and exhaustion, which seemed to foretell death, has, perhaps, originated in a roasted partridge. The apparent paralysis is not infrequently the effect of liquor. Not infrequently, on the other hand, men have worn the name of drunken sot, when an early autopsy showed that disease of the brain had existed for no little time. I knew one of your profession to be the means of sending a patient to a smallpox hospital, whose eruption was caused by the balsam of copaiba, which the druggist had prescribed. The simple medicine does harm, if you do not know how to use it; and, if in no other way, it may produce harm by taking the place of that which might have done service, and the use of which has been put off too long.

Another matter occurs to me, which is worth speaking of. Not that any one of your number is likely to err in this way, for no gentleman can act otherwise than as a gentleman; but you may have those in your employ to whom a word of caution may be useful. No one but the patient or the writer of the prescription knows for what purpose the prescription is written. A look at the patient, as if there were the shadow of knowledge in regard to the disease, is an insult. The slightest intimation of suspicion, that one knows for what disease a particular remedy is prescribed, entitles the seller of the drug to be kicked out of his own door. Yet I have known a druggist to insinuate to a modest girl, to whom copaiba was given for a cough, that he knew for what other disease it was used; and to another, that ergot was not always given to *check* hæmorrhage.

And still another word. The drug store is not the proper place for a news room. It is no place for unemployed physicians to gather in and talk over the political affairs of the day. It is no place for medical or other loafers behind the druggist's counter. No one should be allowed there except upon business. No one should be allowed to speak with him who is weighing out and measuring medicines; and if your visitors do not know this, it would be as well to say it in unmistakable language, or, which would be better, perhaps, to have the compounder of medicines closeted by himself alone.

And now, gentlemen, having said what has occurred to me during the short time which I have been able to spare for the purpose, if I have given you anything which you consider of any value, I shall feel very well satisfied. If these matters appear to you to be of comparatively little consequence now, the day is coming when some of you will appreciate them. It is impossible that science can be held back, and in any business where a head is wanted, as well as a hand, there must be progress. See to it that your labor does not retard it.

LARGE CALCULUS.—The following account of a calculus of gigantic magnitude is copied by a Mr. Gouge, from the preface to an old book of sermons by the Rev. Nicholas Byfield, of Isleworth, who lived in the time of Queen Elizabeth and James I. The book was published, after his death, by the editor, Mr. Gouge, to whom we are indebted for the details of this remarkable case, and is dated 1623.

"It appears that he carried a torturing stone in his bladder fifteen years together and upward. I have heard it credibly reported that, fifteen years before his death, he was by a skilfull chirurgion searched; and that, upon that search, there was a stone found to bee in his bladder; whereupon hee used such meanes as were prescribed to him for his case, and found such help thereby, as he thought; that either the chirurgeon which searcht him was deceived; or that the means which he used, had dissolved the stone. But time which manifesteth all things, shewed, that neither his chirurgeon was deceived, nor yet his stone dissolved; for, it continued to growe bigger and bigger, till at length it came to bee of an incredible greatness. After his death, hee was opened, and the stone taken out; and being weighed, found to be 33 *ounces and more in weight;* and in measure about the edge, *fifteen inches and a halfe;* about the length, *above* 13 *inches;* about the breadth, *almost thirteen inches;* it was of a solid substance; to look upon, like a flint. There are many eie-witnesses besides who can iustifie the truth hereof. A wonderfull work of God it was, that he should bee able to carry such a stone in his bladder, and withall to do the things which he did."—*British Medical Journal.*

CASE OF DOUBLE DIAPHRAGMATIC RUPTURE AND HERNIA.—Dr. John M. Woodworth reports a case of double rupture of the diaphragm to the left of the œsophageal opening. At the autopsy, it was revealed that portions of both the large and small intestine, as well as of the omentum, had been projected into the pleural cavity, and that strangulation of the hernia, with perforation of the intestine, had resulted. As to the previous history, it was ascertained that the man, while engaged in loading ship ten days previous to his death, had attempted to shove some heavy planks up a steep incline, bearing their full weight against his abdomen. The planks were so heavy that sometimes he required assistance, and the gangway being narrow, another sailor endeavored to assist him by pushing from behind. Whilst thus engaged, he felt a sudden snap inside, and, feeling faint, had to suspend work. In a few days, he apparently recovered; but, later, severe abdominal symptoms set in, and death followed.—*New York Medical Journal.*

Progress in Medicine.

REPORT ON OPHTHAMOLOGY.

By O. F. Wadsworth, MD.

The Crossing of Nerve Fibres in the Chiasma.

The theory of the semi-decussation of the optic nerves in the chiasma has, since the time of Wollaston, been generally accepted. With very few exceptions, it explained satisfactorily the alterations in the shape of the field of vision observed in cerebral disease, and particularly did it offer a ready solution (in the affection of one optic tract) of the not infrequent cases of stationary, sharply defined loss of function in the right or left halves of the retinæ. The latter seemed, indeed, hardly understandable on any other supposition. Anatomical proof did not seem to be wanting.

It would appear now as if this theory must be abandoned. Biesiadecki, in 1861 (*Sitzungsberichte der Math.-naturwiss. Classe.* Wien.) convinced himself, by anatomical investigation, that total crossing of the nerve fibres in the chiasma occurred, but his views found no acceptance. Since then, however, testimony in their favor has been accumulating. Paulowski, in 1869, whose work was only published in Russian, arrived at similar results. Gudden (*Archiv für Psychiatrie*, 1870), by experiments on young rabbits, removing a portion of the corpora quadrigemina and thalamus optici on one side, or destroying one retina, found atrophy of one optic tract and the opposite optic nerve, and, with the microscope, demonstrated complete crossing of fibres in the chiasma. Brown-Séquard (*Archives de Physiologie*, March, 1872; *Archives of Scientific and Practical Medicine*, ii., 1873; Journal, November 7, 1872), basing on pathological observations and experiments, denied the truth of Wollaston's theory, but his view would accord still less with total than with semi-decussation. Knoll (*Centralblatt*, 17, 1872; Journal, November, 7, 1872) found reflex contraction of the pupil wanting on one side when the opposite tract was divided; but only inferred total crossing of special fibres which transmit the stimulus from the optic to the oculo-motorius. Finally, Mandelstamm* and Michel (*Graefe's Archiv*, xix. 2), the one in Vienna, the other in Leipsic, have confirmed Biesiadecki's statement by anatomical researches, and find that all the fibres of the optic nerves cross at the chiasma. The independence of their observations, as well as the fact that they employed chiefly different methods, makes the generally very exact agreement of their detailed results all the more convincing. There appear to be, however, certain discrepancies on a few points which, perhaps unavoidable in the working out of a subject so difficult, and partly to be explained by the difference in the methods employed, may yet leave a loophole for doubt for the advocates of semi-decussation. Mandelstamm hardened the human chiasma, and, after dissolving the connective tissue with concentrated solution of potash, picked out the different bundles of nerve fibres with fine forceps. A bundle of nerve fibres at the inner surface of the optic nerve curves downward around the nerve and crosses on the under surface

* For a translation of a part of Mandelstamm's article, by Dr. H. W. Williams, see Journal, March 26, 1874.

of the chiasma, while the outermost of the fibres coming from the tractus to the superior surface of the chiasma curve down around its outer angle and also cross on its lower surface. With this exception, the bundles cross without special change of level; the innermost fibres of the nerve running along the anterior angle of the chiasma to reach the opposite tract, the outermost along the outer to curve and cross at the posterior angle. The more anterior the fibres are situated, the sooner do they cross. Horizontal sections of the hardened chiasma, he regards as suited to assist the judgment arrived at by picking out the fibres, but as not sufficient of themselves to decide the question. Experiments made on rabbits, after the manner of Gudden, gave him like results, i. e., atrophy of nerve and tract of opposite sides.

Michel examined the chiasma in fishes, amphibia, birds, mammals and man by sections, chiefly horizontal, but, also, in other directions. Except in fishes, where the nerves simply overlie each other, he found that in the total crossing of the fibres the nerve bundles interlaced so as to form a sort of basket-work, the size of the interlacing bundles varying in different species, and the most posteriorly situated being smaller than the more anterior in the same chiasma. In man were some layers, also, which crossed without interlacing. The course of the fibres in their passage from one side to the other through the chiasma agrees with the description given by Mandelstamm. In the chiasma of a dog, with congenital malformation of one eye and atrophy of corresponding nerve and opposite tract, the crossing of the atrophied nerve bundles could be plainly traced. The few bundles which Mandelstamm described as curving from upper to lower surface of the chiasma, Michel makes no mention of, but they would very likely escape observation by the method he employed.

Both observers deny the existence of an anterior commissure; and both, while describing a narrow layer of fibres resembling a posterior commissure, state that this layer is separated from the chiasma by a layer of grey substance.

Mandelstamm does not appear to have observed the interlacement of nerve bundles which Michel prominently describes and figures, yet, if the latter is correct, it is difficult to see how they could have escaped notice in the process of picking out and following the various nerve bundles. Mandelstamm, again, states that in the rabbit the one nerve crosses above the other, while Michel found in the rabbit, as in various other mammals, that the bundles of the two nerves formed, in crossing, a regular, delicate basket-work.

The acceptation of a total crossing, Mandelstamm considers suited to fill any gaps at present existing in the determination of the locality of an affection of the brain, and in remarkable accordance with all hitherto observed pathological changes. Without a very great change in our present knowledge of the origin and distribution of the optic nerve fibres, however, this acceptation could never explain the cases of sharply-defined, symmetrical, lateral hemiopia (loss of function in the right or left halves of both retinæ). The less frequent cases of loss of function in the outer sides of both retinæ would, indeed, be more easily explained, since there need only be supposed a lesion affecting one of the outer angles of the chiasma, instead of, as hitherto believed, an affection of both the outer angles, or of the outer side of both tracts or nerves. But such defects of the field of vision, as well as all others,

except the lateral hemiopias, are never stationary, sharply defined and symmetrical. The report of the single case examined *post mortem*, which Mandelstamm has been able to adduce to show that a tumor behind the chiasma produces a loss of function in the outer side of both retinæ, is too defective in important particulars bearing on this point to be of any special value, while a case reported by Knapp (*Archives of Scientific and Practical Medicine*, No. 4, 1873), in which stiff, atheromatous arteries were in contact with both outer sides of chiasma and optic nerves, furnishes *at least* as strong an argument for the old view.

Michel admits the difficulties in the way of explaining the sharply defined lateral hemiopias with total crossing of fibres, and does not attempt any solution of the problem. He describes a pouch, covering the upper surface of the chiasma and separated from it by a thin layer of grey matter, which communicates with the third ventricle, and points the importance this pouch may have in the production of loss of function in more or less of the nerve fibres when there is a collection of fluid in the ventricles.

The practical importance of the views above given is sufficient to stimulate inquiry, and it cannot be doubted they will soon be confirmed or denied by other workers in the same field.

Inflammation of the Cornea.

Eberth (*Centralblatt*, xix. and xxxii., 1873), in the former of his papers, sought to prove, from the results of experiments, that the severity of inflammatory processes in the cornea, after a trauma, does not depend so much on the severity of the wound itself as on the number of bacteria carried into the cornea by the wounding body. He found, also, that the changes produced when a silken thread was left in the cornea were the same as when the cornea was inoculated with the bacteria from diphtheria of the throat, and deduced identity of the disease. From another series of experiments, he concludes that the presence of similar organisms forms an essential part of the keratitis following paralysis or section of the fifth nerve. After section of this nerve in rabbits, there occurs slight exophthalmos, diminished sensibility of cornea and less frequent winking; the drying of the exposed surface of the uncovered cornea causes a change in the epithelium and allows the organisms floating in the air to gain a foothold. The keratitis which follows does not differ, to the eye, from true diphtheritis of the cornea, and the microscope shows, as in diphtheritis, bacteria, scattered and in groups, in the affected tissues. The differences in degree and rapidity with which the changes in the cornea occur depend on the rapidity and extent of drying of the surface, on the presence or absence of slight injuries causing abrasion of the epithelium, and on the amount of micrococci in the air.

The results of investigations by Stromeyer, as to the causes of hypopion-keratitis (*Graefe's Archiv*, xix., 2), point also to the influence of bacteria. Bits of metal, carefully cleaned, and introduced into the cornea so as to allow the wound to readily close behind them, excited but little inflammation, and that in proportion to the readiness with which oxydation from contact with the corneal fluids took place. Septic material, containing bacteria, leptothrix buccalis and decaying muscle, placed between the lamellæ of the cornea excited rapid and

extensive keratitis, with extension of the inflammation to the scleral border, ciliary body and iris, and hypopion. The same series of changes sometimes occurred when perfectly fresh muscle substance was employed; sometimes the inflammatory reaction was slight. Examination showed that, in the former instances, the bits of muscle had become rapidly infiltrated with bacteria, while, in the latter, closure of the external wound had prevented their introduction. In one case, where a bit of fresh muscle was introduced into the anterior chamber, its presence was perfectly tolerated, and, at the end of three weeks, it was in great part absorbed. The cases of keratitis in persons affected with blenorrhœa of the lachrymal sac present strong points of resemblance to keratitis produced by septic inoculation. The source of the pus forming the hypopion is that portion of the ciliary body situated in the angle of the anterior chamber and the circulus venosus of Leber; very probably, also, in many cases, the iris takes part in its production. The anatomical appearances found negative decidedly the opinion that hypopion occurs by sinking of pus corpuscles in the posterior lamellæ of the cornea behind the membrane of Descemet, or that they reach the anterior chamber by direct passage backward from the seat of ulceration.

Boettcher (*Virchow's Archiv*, 58, 3), believing that the emigration of white blood-corpuscles into the cornea, which Cohnheim and others have described in traumatic keratitis, was due to the great amount of irritation excited, sought to confine the destruction of tissue to as small and central a spot as possible. When this was effected, he found no evidence of emigration of white corpuscles, and while the corneal corpuscles were destroyed in the immediate vicinity of the eschar formed, those in the peripheral parts showed no notable change. In the injured part, fissures appear in the basis substance, containing fine granules, which gradually enlarge and become pus corpuscles. The pus cells, therefore, originate on the spot by free cell-formation from protoplasma granules brought to the seat of injury in the increased amount of nutritive fluid attracted by the irritation.

The experiments of Sinitzin (JOURNAL, November 7, 1872), which appeared to demonstrate that keratitis following section of the fifth nerve is averted by removal of the upper cervical ganglion of the sympathetic, have been repeated by Eckhard (*Centralblatt*, 35, 1873) with a contrary result. In only one case did the characteristic changes in the eye fail to follow division of the fifth after removal of the cervical ganglion. In the solitary exception, it was found that the median fibres of the trigeminus had escaped division, though complete anæsthesia of the cornea had been produced.

Reich, also (*Graefe's Archiv*, xix., 3), in the course of experiments to determine the nerves presiding over the secretion of tears, found that extirpation of the upper cervical ganglion of the sympathetic did not prevent neuro-paralytic keratitis after division of the trigeminus.

Von Tannhofer (*Central-Zeitung*, 46, 1873) describes the nerves of the cornea as running in lymph-canals lined with endothelium, and states that, in keratitis, the white blood-corpuscles wander along these canals, often by their number completely hiding the nerves from sight. He believes, also, that he has been able to trace the anatomical continuity of the fine terminations of the nerves with corneal corpuscles.

[To be concluded.]

Bibliographical Notices.

Annual Report of the Supervising Surgeon of the Marine Hospital Service of the United States, for the Fiscal Year 1873. By JOHN M. WOODWORTH, M.D. Washington: Government Printing Office. 1873. Pp. 145.

THIS is an elaborate document, and one well worthy of an extended review. It is Dr. Woodworth's "second annual report," and it evinces not only a thorough competency for the work he has to do as "Supervising Surgeon of the Marine Hospital Service of the United States," but also a genuine, painstaking and careful examination of the subjects brought forward, forming an admirable and valuable *exposé* of the details of the important department referred to. Any analysis fully setting forth the merits of the Report would be impossible within the limits assigned us. A reference to its main topics will, however, serve to show its value and the large amount of labor required of the Supervisor.

In a circular accompanying the Report, Dr. Woodworth shows, in a few sentences, both the importance and the extent of the Marine Hospital Service, and enumerates the duties attaching to his post. He says:—"For the purpose of illustration, rather than of comparison, the Marine Hospital Service of the United States may be said to be analogous in its functions to the Medical Departments of the Army and the Navy, in that it is the medium through which medical and surgical treatment is furnished to the sick and disabled of a large body of men. Its scope, however, is wider than that of those Departments; as well in the variety of duties imposed by law upon the chief of the service, as in the much larger number of men cared for through it." The mere figures given by the reporter at once show the truth of these assertions. Thus there are, "in round numbers," about 113,000 men "constituting the American merchant marine, comprising the officers and crews of 26,852 vessels, of 3,721,425 tons burthen." A portion of the act passed June 29, 1870, reorganizing the Marine Hospital Service, prescribes a list of duties for the Supervisor, which, as he truly remarks, "in addition to those which usually appertain to a medical department, include, also, to a considerable extent, the functions of the paymaster's, commissary's and quartermaster's departments." *(Loc. cit.)* Here is a portion of the act, or law, referred to; it speaks for itself, and it asks a great deal: "the Supervising Surgeon, 'under the direction of the Secretary' [of the Treasury], should not only secure suitable medical attendance for the sick and disabled, inspect hospitals—their construction, condition, administration, &c.—and, in kindred ways, exercise a strictly professional knowledge and authority, but he should aim to secure a diligent collection of hospital dues [assessed upon the individual members of the merchant marine, monthly—thus forming a 'marine hospital fund'], and the equitable enforcement of the act with regard thereto; he should become familiar with prices-current of medical, subsistence and other stores, and with the relative conditions and cost of maintenance, &c., throughout the country, in order to audit accounts, revise requisitions, and determine the necessary amount and character of relief to be furnished at the various ports—in short, he should, literally, 'supervise all matters connected with the marine-hospital service, and with the disbursement of the fund.'" Might not the Supervisor, after reading the above, reasonably exclaim—"*Ohe! jam satis?*"

Now, what has Dr. Woodworth presented in the one hundred and forty-five pages of his Report? In the report proper, he has detailed the doings of the service from July, 1872, to June 30, 1873; the progress of the re-organization of the service; regulations of the United States Marine Hospital service (1873); existing laws—prospective legislation; tax-paying seamen in quarantine hospitals; medical inspection of seamen before shipping; United States Marine Hospital buildings; proposed new hospitals; statistics. Under the latter head are comprised the fiscal and local statements, a "table of

diseases and injuries treated in hospital during the fiscal year 1873," and a "mortality table, with causes." After this, as an appendix, we have a collection of truly valuable papers—on hospitals and hospital construction; on the natural history of yellow fever in the United States; report of a case of double diaphragmatic rupture and hernia; urethral stricture; the sailor and the service at the port of New York; river boatmen of the lower Mississippi. To say that each of these short essays is full of interest and valuable information, is giving them only a minimum of praise. The paper on hospitals, by Dr. Woodworth, which he modestly speaks of as a mere "outline of the subject," is well worthy of perusal, and advocates "the pavilion hospital plan," which, as he remarks, "inaugurates a new era in hospital construction, at least, so far as the marine-hospital service is concerned." Valuable illustrations of hospital-plans accompany this paper.

The next paper—that upon yellow fever—is by J. M. Toner, M.D., and is prefaced by a "chart of yellow fever in the United States, giving elevations above sea-level of localities where yellow fever has appeared since A.D. 1668." In addition to the preparation of this chart, Dr. Toner has given a large number of meteorological and locality tables. These occupy several pages, and must have required a great deal of patient labor in their construction. This paper closes with an account of "the yellow fever epidemic of 1873," whose ravages in many southern and western localities—particularly in Shreveport, La.—have been so appalling. The statements from Shreveport had not been obtained at the time Dr. Toner made up his report, in consequence of the illness of the surgeon in charge.

The paper upon the case of double diaphragmatic rupture and hernia is by Thomas T. Minor, M.D., and is accompanied by a photographic illustration. The account is exceedingly interesting. Reference is made to Dr. Bowditch's treatise on diaphragmatic hernia, upon the point of recognition of these rare cases during life—Dr. B., at the time of his writing, knowing only two instances of such recognition, one by Mr. William Lawrence, of London, the other by himself, at the Massachusetts General Hospital. Dr. Minor expresses the opinion that "there appears to be, aside from want of familiarity, owing to the infrequent occurrence of this form of hernia, no reason why the diagnosis may not be made with tolerable accuracy in any case where the attention is awakened, as by the history of a previous severe injury of the trunk." He allades, in this connection, to the frequency with which sailors meet with such injuries, and urges the necessity of careful scrutiny of the chest and abdomen by marine-hospital surgeons in such cases; believing that many instances of herniæ of the sort under consideration may be overlooked.

A short report upon stricture of the urethra, by C. H. Ellinwood, M.D., Surgeon U. S. Marine Hospital Service, San Francisco, Cal., is next in order, and is accompanied with notes of cases by Dr. Ellinwood and Dr. O. L. Crampton, Surgeon in charge of United States Marine Hospital, Mobile, Ala. The "divulsion" method of Gouley finds favor with Dr. Ellinwood. He refers to the well-known fact that many cases of stricture in seamen are aggravated by delay in securing proper treatment, from their own fault, most frequently; and also mentions the injury inflicted by "the introduction of improper instruments, mostly in the hands of the seaman himself"—a fertile source of urinary fistulæ.

The next paper is entitled "The Sailor and the Service at the Port of New York," and is by Heber Smith, M.D., Surgeon United States Marine Hospital Service, New York city. The abuses and cruel impositions and robberies practised upon the sailor when he comes ashore, are forcibly set forth by Dr. Smith, who fully realizes the duties of surgeons holding the important relation to seamen which attaches to the marine hospital service. We commend this paper to the careful consideration of legislators, and of all others who can in any way aid in abating the abominable abuses practised upon sailors by boarding-house keepers and their satellites. In replying to his own query, "what is the remedy for this state of affairs?", Dr. Smith finally concludes that "the general government only can cope with the

difficulty." We trust the faithful exposure, by Dr. Smith, of the hardships and injustice inflicted upon the sailor, not only in the "port of New York," but everywhere, more or less, will engage the best attention and earnest intervention of those in authority.

The last paper—somewhat analogous to the previous one—occupies only three pages of the pamphlet, but they well deserve careful attention. The title is, "Report on the River Boatmen of the Lower Mississippi"; the author is Orsamus Smith, M.D., Surgeon United States Marine Hospital Service, New Orleans, La. The report is "an investigation into the mode of life, food, shelter, and other conditions affecting the health of seamen on river-boats." Referring to the cotton-boats, the reporter says:—"In view of their construction and of the hardships their crews endure from exposure, through want of accommodation, together with the unusually severe winters which have been experienced in the south for the past two seasons, it is no wonder that such diseases as smallpox, rheumatism, and pulmonary complaints are very prevalent. To these causes of disease should be added the immense growth of rank vegetation in these low, swampy regions, which, when decaying in the late summer and fall seasons, during low water, is a very fruitful source of a large percentage of every form of malarial fevers."

A good Index, with a "List of Illustrations," closes this excellent pamphlet. W. W. M.

Practical Treatise on the Diseases of Children. By J. FORSYTH MEIGS, M.D., and WILLIAM PEPPER, M.D. Fifth Edition, revised and enlarged. Philadelphia: Lindsay & Blakiston. 1874.

THE fourth edition of this well-known and excellent work has been for some time exhausted, and the demand for a fifth edition so soon shows the high estimation in which it is held by the profession. Any extended notice, at this time, of a book possessing so well-merited a reputation would be superfluous. In this edition, there are to be found several changes. Some important articles, those on Diseases of the Heart, on Progressive Muscular Sclerosis, on the Treatment of Scarlet Fever and of Measles, on Variola and the Vaccine Disease, have been almost entirely re-written. Other subjects, of which no mention was made in previous editions, have been introduced. Such are: articles on Pulmonary Emphysema, Pneumothorax, Affections of the Tonsils, Retro-pharyngeal Abscess, Malarial Fevers and Scrofula. Notwithstanding in many parts there has been much condensation of the text, the large amount of new matter added has caused an increase of ninety pages.

The aims of the authors to insure a practical character to the work have been faithfully carried out, and, without neglecting other questions, a very large space is devoted to the considerations of treatment, which are clearly stated, with much attention to detailed instructions.

To the chapter devoted to entero-colitis is added a table showing the monthly mortality for the last seven years from cholera infantum, dysentery and diarrhœa during the first five years of life, compared with the total monthly mortality from other causes, and the mean monthly temperature, prepared with great care, showing very strikingly how great is the effect of season and how great a factor the elevated temperature of summer is in the production of this fatal intestinal disease; considerable space is also devoted to showing how strongly allied it is in its nature and pathology to the camp diarrhœa described by Dr. Woodward, of the U. S. Army. In this edition, the term cholera infantum is restricted to that class of cases having a truly choleraic character, and, if such definition were adopted generally by American practitioners, the rates of mortality from this disease would not be so large as they now appear in statistics. Under the head of diseases of the digestive organs, is a very excellent article upon diseases of the cæcum and of the appendix vermiformis, a subject not usually specially described in works upon children's diseases.

Highly valuable and instructive are the authors' views, given at length, to which they have arrived by long experience, upon several of the most impor-

tant articles in the materia medica, viz.: calomel, antimony, opium and alcoholic stimulants. While the two former they have nearly discarded, in the treatment of very young children, as being, in the majority of cases, useless if not productive of much harm, their convictions of the great value of the two latter, when properly and understandingly administered, have been only strengthened by time.

This work, though not destined to supersede foreign treatises upon children's diseases, as those of West, Vogel and Steiner, is well worthy to be placed by their side, and, at the same time that it shows the authors to be conversant with the advanced ideas of pathology and therapeutics of foreign schools of medicine, it offers itself as an exponent of the best American thought and practice.

The admirable arrangement adapts it equally well as a book of reference for the busy practitioner and as a text book for the student.

L'Hérédité. Par TH. RIBOT. Paris. 1873. Pp. 550. 8vo.

THIS treatise of M. Ribot deals with mental heredity especially. The four parts treat of its phenomena, laws, causes and consequences. In the first, the author shows the heredity of the instincts, sensorial faculties, memory, imagination, intelligence, passions, will, national character, and morbid psychological conditions. His tables of illustration are profuse and convincing, and concern, in many cases, historical personages, being taken from Galton's work on Hereditary Genius, from Lucas, Despine, Darwin, Spencer and many others.

From this mass of facts, he educes the general law of mental heredity in three forms: direct, as from parents to children; atavism, as from grandparents or remoter ancestors, skipping one or more generations; and indirect or collateral. This law is subject to so many exceptions, however, as to almost warrant a belief in what Lucas termed the law of individualism *(innéité)*. He concludes that there is no such law, since the exceptions are always partial. The mental characteristics of the species are transmitted, of course, and the deviations from family traits of mind are, he thinks, accidental, due to transient mental states of the parents, to intra-uterine influences, &c. Many seeming exceptions are metamorphoses of heredity, whereby mental traits combine like two. chemicals, forming a compound different from either. We see this in transmitted insanity, in which not the precise form of mental disease is inherited, but some morbid equivalent. Heredity, then, is the law, but a law which, from a complexity of causes, can never completely realize its ideal.

The consideration of the causes of mental heredity involves a critical examination of the physical and mental elements in man. As Maudsley, Carpenter and others have done, our author first shows that every mental operation may be performed unconsciously. He next, after Spencer, shows how every conscious mental act may be resolved into a nervous shock, which, seen from below, is a physical, and, from above, is a mental phenomenon. Hence result two contemporary hypotheses, best known as materialistic and idealistic, neither being provable, but the former to be preferred, since it starts with the known and tangible, and leads up to the ideal and abstract. He therefore argues that every psychological has a corresponding physiological state, and that mental heredity is, therefore, dependent on physical heredity. To explain this, he offers Darwin's theory of *pangenesis*, which implies that in the whole organism each atom or unit composing it reproduces itself. Each cell emits little germs, which ultimately develope in the body of parent or child or grandchild into similar cells, with similar organic tendencies. Heredity is, therefore, in fact, *identity*, partial, of course, and subject to variations under the laws of evolution, which are unfolded in the fourth part, in which the consequences are treated of.

The doctrine of heredity is essential to the evolution hypothesis, since by an accumulation of little differences, it becomes a means of selection. It may, however, favor the degeneracy as well as the amelioration of any race,

unless the surroundings are favorable to improvement. In the long run, and in the human species at large, it undoubtedly leads to progress. It favors improved physical conditions and an hereditary accumulation of advanced ideas and forms of thought, of nobler sentiments and a higher moral standard. Whether man can ever use and direct this powerful agent for the betterment of his species, is a problem for the future. Its importance in a pathological view is apparent, and if the same zeal was shown for the improvement of a human race or a family as for that of some valuable breed of cattle, similar results, no doubt, would follow. T. W. F.

BOOKS AND PAMPHLETS RECEIVED.

Forty-eighth Annual Report of the Massachusetts Charitable Eye and Ear Infirmary. 1874.

Annual Report of State Lunatic Asylum, Utica, N. Y., for 1873. Albany. 1874. Pp. 60.

A Manual of Toxicology, including the consideration of the Nature, Properties, Effects and Means of Detection of Poisons. By John J. Reese, M.D. Philadelphia: J. B. Lippincott & Co. 1874. Pp. 507. (For sale by A. Williams & Co.)

Ligation of Arteries. By Dr. L. H. Farabeuf. Translated by John D. Jackson, M.D. Philadelphia: J. B. Lippincott & Co. 1874. Pp. 157. (For sale by A. Williams & Co.)

Lectures on the Diseases of Infancy and Childhood. By Charles West, M.D. Fifth American from the sixth revised and enlarged English Edition. Philadelphia: Henry C. Lea. 1874. Pp. 678.

A Treatise on Pharmacy. By Edward Parrish. Fourth Edition, enlarged and thoroughly revised, by Thos. S. Wiegand. Philadelphia: Henry C. Lea. 1874. Pp. 977. (For sale by A. Williams & Co.)

The Treatment of Syphilitic Diseases by the Mercurial Vapor Bath. By Langston Parker, F.R.C.S.L. Compiled from the fifth London Edition, by John W. Foye, M.D. Boston: A. Williams & Co. 1874. Pp. 92.

Extract from a Report on the History of the Surgery of Tennessee. By Wm. T. Briggs, M.D. (From the *Nashville Journal of Medicine and Surgery.)* 1874. Pp. 98.

Therapeutics, Materia Medica and Toxicology. By H. C. Wood, Jr., M.D. Philadelphia: J. B. Lippincott & Co. 1874. Pp. 578. (For sale by A. Williams & Co.)

RETENTION OF URINE RELIEVED BY ASPIRATION.—Dr. Joseph Bell, in a paper read before the Medico-Chirurgical Society of Edinburgh, expresses the opinion that the aspirator affords an easy, safe and reliable means of emptying the bladder in cases of urgent retention of urine, where catheterization has failed. He reports a case in which this operation was resorted to in a feeble old man, who was admitted to the hospital in a semi-comatose state, with eyelids swollen and limbs œdematous. He had a weak, rapid pulse and a harassing cough, along with the physical signs of bronchitis and emphysema. His scrotum and penis were enormously swollen, and of a dark-red color. There were several sinuses in the scrotum and perineum. He had passed no urine by the urethra for three days, but a few drops had dribbled away by the sinuses in the perineum. His bladder could be felt distended nearly up to the umbilicus. It was not deemed prudent, considering the unpromising condition of the patient, to perform perineal section, and tapping *per rectum* would have been difficult, on account of the enlargement of the prostate. The needle of Dieulafoy's aspirator was therefore driven into the bladder about one inch and a half above the symphysis pubis, upon the median line, and by it twenty ounces of fœtid, turbid urine drawn off with ease. No trace of the puncture was visible after the first few days, nor did it occasion the slightest inconvenience.—*Edinburgh Medical Journal*, April, 1874.

Boston Medical and Surgical Journal.

Boston: Thursday, May 7, 1874.

We look forward with much interest to the experiment of Hospital Sunday. It is of great importance that the first trial should be a success, or, if it is not, that the cause of the failure should be so evident that it may be removed on future occasions. The idea, certainly, is good in the abstract. Our hospitals, as a rule, are in narrow circumstances; so much so, indeed, that the proceeds of the collections will be but as a drop in the bucket. Most of our hospitals are dependent on the charity of the rich; it is by this favored class that wards are built and endowed, instruments purchased, and beds maintained. This is as it should be, but we think there is something pleasing in the idea of once a year giving the poor an opportunity to repay in some measure the benefits they have received, by contributing their mite together with their more fortunate neighbors. This will give them, and very justly, a feeling of ownership and a personal interest in institutions to which accident may, at almost any time, consign them, and will tend to do away with that dread of the hospital which is still too common. Moreover, this charity will take the form in which it is most acceptable; that of a free gift. Half the benefits of princely bequests are often lost by conditions which in some cases may even make their receipt a burden. Here the form of the gift is good, but we are sorry to learn that abroad much trouble has arisen in dividing it, and that the result has been by no means satisfactory. The *London Medical Times and Gazette*, April 11, 1874, goes so far as to speak as follows:—

"Whilst giving the originators of the new project every credit for their motives in instituting a Hospital Sunday or Saturday collection, we think that in the long run it will be found the new system is open to many objections which did not exist under the old arrangement, and we are of opinion that the interests of charity in this particular direction would by no means suffer if "Hospital Sundays" and "Hospital Saturdays" were done away with, so far as the metropolis itself is concerned, and the funds of the various hospitals of London were left to be recruited in the manner which has been adopted for so many years past."

We are inclined to hope for better things, though we are aware that serious difficulties may arise. The clergymen composing the committee that has the matter in charge are doubtless better fitted by their charity and eloquence to raise a large sum than by their knowledge of medical matters to divide it when raised. We hope they will bear in

mind that corporations resemble individuals, and that those who are least deserving are precisely the ones to press their claims with the greatest effrontery. The task of the committee is a difficult and delicate one, especially for those who cannot know the relative merits of the numerous institutions that will appear as claimants. We would suggest to the committee that it might not only lighten its labors, but further the end for which it exists, by requesting the Suffolk District branch of the Massachusetts Medical Society to appoint a committee of conference to advise as to the proper distribution of the money. It is not too late to adopt this course, even for the present year, and thus avert the only danger that seriously threatens the success of what should be a most popular charity.

CLERGYMEN'S SORE THROAT.—In an address upon Clerical Elocution, delivered some years since, Edward S. Gould, Esq., made some ingenious and valuable suggestions as to the injurious effects upon the voice produced by the vicious habit of monotonous delivery to which clergymen are addicted. These ideas we do not remember to have seen elsewhere in print, and as they appear to be based upon correct physiological principles, we would recommend them to the consideration of those medical men who are inclined to consider a European tour as the sovereign panacea for affections of the throat in clergymen:—

"You are all aware, from your own observation, that a man can talk through a whole evening with less fatigue to his vocal organs than would ensue from his reading half an hour. Why is this? The human voice is like a stringed instrument of music. It has a certain number—more or less in individual cases—of tones, or notes, and, in common conversation, almost every one of these notes is brought into use; and, when used properly, each is kept in order, and even improved, by use. But if two or three only are used, as in monotonous reading, they become fatigued and permanently injured by being used too much; while the others become rusty, weak, discordant, from not being used at all. In other words, and to drop the metaphor, the diseases of the throat, so common among the clergy, are easily and directly traced to their habit of monotonous delivery."

As every one knows, chemical nomenclature has gone into the line of the acrobat, and in the last few years somersaulted most actively. As no one seems to know, the United States Pharmacopœia has, to a certain extent, followed these changes. It is now no longer correct to write magnesiæ sulphas, or sodæ sulphas, or potassæ nitras. The name of the metal itself is employed, not that of its oxide. Thus, the substances mentioned should be respectively called magnes*ii* sulphas, sod*ii* sulphas, potass*ii* sulphas.—*Philadelphia Medical Times.*

The Hospitals.

MASSACHUSETTS GENERAL HOSPITAL.

(Wednesday and Saturday, April 22 and 25, 1874.)

OPERATIONS were performed in the following cases:—Stricture of the Urethra, Supra-orbital Cyst, Sinuses of Back, Cellulitis of Forearm, Hæmorrhoids, Necrosis of Femur, Hæmorrhoids, Supra-orbital Cyst, Spermato-

cele, Supra-orbital Cyst. During the week, Cancer of the Breast, Phimosis, Double Talipes Varus, Elongated Prepuce, Railroad Injury of Foot and Ankle-joint, Tumor of both Upper Jaws, Hydrocele.

Stricture of Urethra.—Case reported in the JOURNAL for April 30th; under ether, another attempt to enter the bladder through the stricture resulted successfully. Dilated with the largest sized Voillemier tube, and a No. 12 elastic catheter left in the bladder.

Cyst—congenital, of the supra-orbital region, in a middle-aged man. Excised by Dr. Clark.

Sinuses of Back—in a boy twelve years old. He was struck, about a year ago, in the lumbar region by a stone. A deep-seated abscess followed, which opened spontaneously below the lowest rib of the right side, leaving a fistulous opening. A second fistula exists two inches from the one just mentioned, near the crest of the ilium; the two communicate deeply with a large cavity in the iliac region. The sinuses were laid open and a tent inserted, to facilitate drainage. It was remarked that the location of the abscess and its history suggested the possibility of its being peri-nephritic in character.

Cellulitis of Forearm—in a middle-aged man, following an amputation of the hand, through the metacarpal bones, for an extensive lacerated wound. The original injury, by stripping some of the muscles from their sheaths, had opened the way for the diffusion of the inflammatory processes from the hand to the elbow; pus had burrowed and infiltrated the tissues in all directions, and required the freest incision to afford adequate drainage.

Hæmorrhoids—in a man forty-nine years old. Attention was directed to the anæmic condition of the patient, the result of frequent and profuse hæmorrhages. Ligatured.

Necrosis of Femur—of ten years' duration, in a man thirty years old. Fistulous openings on the outer and inner aspects of the thigh communicated with dead bone in the lower third. The denuded surface was extensive, but a careful probing corroborated the statement made before examining the limb, that it was exceptional to find loose sequestra in necrosis of the femur.

Hæmorrhoids—in a middle-aged man. One mass, which had become consolidated by long-continued inflammation, was excised; the remaining piles were tied.

Supra-orbital Cyst—congenital, in a young woman. The tumor was unusually large, the sac very thin, and adherent to the periosteum; the surface of bone on which it rested presented a corresponding concavity. It was excised and the contents were a tenacious and honey-colored fluid, mixed with hairs; its interior surface was cutaneous.

Spermatocele.—Two operations on this patient were reported in the JOURNAL for April 9th; since then, the sac has again filled. It was tapped once more, and a clear fluid, resembling that usually withdrawn from a hydrocele, was evacuated. The testicles were found to be somewhat enlarged.

Supra-orbital Cyst—congenital, in a young boy. It was excised, and presented the usual appearances of such a growth.

Cancer of Breast—of two years' duration in a woman sixty-two years old. Its growth had been very rapid with very little pain; the whole breast was involved, the skin adherent, and the nipple retracted. Excision of the entire breast was performed by a large T incision; the latter was extended to the axilla, to permit the enucleation of some enlarged glands.

Phimosis—in a young man, caused by chancroids of the glans penis. Circumcision.

Double Talipes Varus—in a child three years old. Tenotomy of the tendo Achillis and plantar fascia of both limbs.

Elongated Prepuce—in a young man; the source of great discomfort and constant irritation to the glans. Circumcision.

R. R. Injury to Foot and Ankle-joint—in a man. The bones of the foot and ankle-joint were comminuted, and the soft parts extensively lacerated and bruised. Dr. Clark amputated at the middle of the leg by the double flap method.

Tumor of both Upper Jaws—painless, and of three years' growth, in a man

fifty-one years old. It involved the alveolar processes of both bones, extending from the second molar tooth of the right side around nearly to the same point on the left, and extending upwards nearly to the infra-orbital foramen. It presented a firm, nodular surface, projecting an inch or more into the mouth from the hard palate and alveolar surfaces, but did not extend into either antrum or the nasal cavities. A straight incision was made through the upper lip into the right nostril, and afterwards extended around the septum into the left; the cheek was dissected up on each side as high as the infra-orbital foramen, both second molar teeth extracted and the soft palate divided transversely behind the tumor. The palate and alveolar processes were then sawn through upwards quite into the antra. The anterior wall of the antra and the nasal processes were detached by a few strokes with the mallet and chisel, the vomer divided by bone forceps, and the tumor easily depressed and removed. Two large mucous polypi, which filled the nasal fossæ posteriorly, and entirely independent of the tumor, were removed by polypus forceps, the ligatures all cut short, and the flaps approximated by sutures.

Hydrocele—of tunica vaginalis in a middle-aged man, tapped, and a seton inserted.

H. H. A. BEACH.

Obituary.

DR. BENJAMIN MANN.

DR. MANN was born in Randolph, Massachusetts, in 1814. He graduated at Amherst College in 1837. His preliminary medical studies were pursued with Dr. Alden, of Randolph, and with Drs. Perry and Bowditch, of Boston. He received his medical degree at the Harvard School in 1840.

His first field of practice was in Foxboro', Mass.; but after twelve years of residence there, he removed to Roxbury, where he continued in active professional labor till obliged by disability to desist. He was on his way home from a journey to Florida when the fatal attack overtook him in Brooklyn, N. Y., on the 21st ult.

Dr. Mann was devoted to the practical element in his profession. He worked with great assiduity, even to the time of life at which physicians generally begin to release themselves from the toils and anxieties of practice, and to seek for rest.

DR. AUGUSTUS HARRIS.

DR. HARRIS died in Colebrook, N. H., on the 20th of April last. His death was the result of a misadventure, whereby he took a tablespoonful of tincture of aconite root, when he intended to take tincture of rhubarb, for a trifling illness. Death occurred in forty minutes after the accidental dose.

Dr. Harris was one of the best known physicians in the upper Connecticut valley, his residence in Colebrook having extended over a period of forty-six years. His medical education was obtained at Bowdoin, and at Dartmouth, from which latter school he obtained his degree in 1838. His practice extended many miles along the Connecticut valley in both New Hampshire and Vermont. He was prominently identified with the White Mountain Medical Association, and was one of its officers. He was active and influential in all public matters relating to his section. As a member of society, he maintained a character above reproach, and his death will be severely felt by the people of Colebrook and vicinity. He became acquainted, professionally and otherwise, with many tourists to the upper Coos country, who will cherish pleasant recollections of him as a faithful physician and as a good and kind man. His age at death was 63 years.

Medical Miscellany.

MEDICINE AT THE ANTIPODES.—The University of Otago, New Zealand, has instituted a chair of Anatomy and Physiology, to which is attached a salary of $3,000 a year, besides class fees.

A VICTIM OF CHOLERA.—The great artist, Wilhelm Kaulbach, recently died of cholera, in Munich. The disease has been exceptionally severe in the Bavarian capital, owing to the water employed in the manufacture of beer—so say the local physicians.—*Lancet.*

APPOINTMENT.—Dr. F. W. Draper has been appointed Visiting Physician to the Boston City Hospital, vice Dr. William Read, resigned.

THE NEW YORK SOCIETY OF NEUROLOGY AND ELECTROLOGY was incorporated last January. Its object is "the study of the anatomy, physiology, pathology and therapeutics of the nervous system, and of electricity in its relations to physics, physiology and therapeutics." A glance at the list of members shows that much may be expected from this Society.

LEAD IN THE BRAIN.—Troisier has demonstrated, by chemical analysis, the presence of this metal, in considerable quantity, in the brain of a man, who, for thirty years, had followed an occupation which brought him in constant contact with lead. There had been no evidence of lead poisoning during life, nor of any brain symptoms attributable to the presence of this foreign agent.—*Le Mouvement Médical.*

WORCESTER NORTH DISTRICT MEDICAL SOCIETY.—At the annual meeting, April 25th, the following officers were elected:—

President.—Ira Russell.
Vice President.—George Jewett.
Secretary.—George D. Colony.
Treasurer.—Alfred Miller.
Librarian.—J. E. Gendron.
Commissioner on Trials.—George Jewett.
Censors.—Alfred Miller, C. C. Field, J. M. Blood, F. W. Russell, E. J. Sawyer.
Councillors.—Ira Russell, George D. Colony, George Jewett, B. H. Hartwell.

BERKSHIRE DISTRICT MEDICAL SOCIETY.—At the annual meeting, April 29th, the following officers were elected:—

President.—C. C. Holcombe.
Vice President.—W. W. Leavitt.
Secretary.—J. F. Alleyne Adams.
Treasurer.—Henry Eastman.
Commissioner on Trials.—Abner M. Smith.
Censors.—C. T. Collins, W. M. Mercer, F. K. Paddock, C. E. Heath, S. P. Dresser.
Councillors.—C. T. Collins, J. L. Miller, W. W. Leavitt, N. S. Babbitt, Andrew M. Smith, J. F. A. Adams.

ANEURISM OF THE ARCH OF THE AORTA.

Patient cannot swallow food
So well, he says, as once he could.
Œdema, pain, sometimes pulsation,
Something wrong with respiration.
Five may-be causes for dyspnœa;—
Veins, vagus, bronchus, lung, trachea.
If these are by the tumor pressed
The breathing will be much distressed.

—*Guy's Hospital Gazette.*

PHYSICIANS' POCKET CASE RECORD AND OFFICE CASE RECORD.—We have received copies of these books, which differ only in size. They contain spaces for the "date, name, diagnosis, age and physique of the patient; the condition of the pulse, temperature, respiration, tongue, urine and stools, also for general remarks." They appear to be very convenient. They are for sale by James Campbell, 18 Tremont St.

NOTES AND QUERIES.

KOUMISS.

CANON CITY, Colorado, April 29, 1874.

MESSRS. EDITORS,—I notice in your JOURNAL of March 26th, page 306, a new medicine, koumiss, referred to as a specific in phthisis. Please inform me as to where I can obtain this remedy, and, if not too much trouble, please give me a little more information as to the nature of the article. Respectfully, J. L. PRENTISS.

Koumiss is the fermented milk of any animal, but usually of the mare. It is said to be very nutritious and easily assimilated. We are not aware that it can be obtained in this country. Consult *British Medical Journal*, Feb. 21 and March 7, 1874, and *London Medical Record*, April 1st.—EDS.

PSORIASIS.

MARION, Ohio, April 29, 1874.

MESSRS. EDITORS,—Will some one of the profession who is posted on diseases of the skin give to the numerous readers of the JOURNAL a rational and philosophical treatment of the disease or diseases, lepra, psoriasis, or tetter. Are they all the same disease, or are they separate diseases, requiring different treatment? To make the fine points drawn out by Willan, is rather beyond my comprehension. Disease of the above character is in this country becoming more prevalent as the country becomes more thickly populated; and in a practice of twenty-three years I have found more cases of tetter, itch, or whatever it is, in the last few years than in all previous years together.

A CONSTANT READER.

A CORRESPONDENT relates the following case, and asks what was the cause of death.

"Woman in labor with eighth child, first seven living and born after easy labors, weighing from twelve to fifteen pounds. The present labor commenced at 12, noon; life felt until 1.30; that she distinctly remembered. I was called at 2.30; found the membranes just ruptured, and at 3 the head engaged in the upper strait in the first position, and at 4 the child was born; as easy and regular a labor as I ever attended for a thirteen-pound child, or any other, in fact.

"The child was perfectly formed; no signs of maceration, but *dead;* not the least pulsation at navel, and resisting attempts at resuscitation, lasting three quarters of an hour. The cord was very small and smooth, greenish in color and very tender. The placenta came away soon, and the patient made a good recovery.

"She felt life from the middle of the fourth month up to two hours before delivery, and not a sign during her whole term differed from the other seven."

MORTALITY IN MASSACHUSETTS.—*Deaths in eighteen Cities and Towns for the week ending April 25, 1874.*

Boston, 163; Worcester, 15; Lowell, 25; Milford, 4; Chelsea, 6; Cambridge, 16; Salem, 10; Lawrence, 10; Springfield, 8; Lynn, 23; Fitchburg, 2; Taunton, 11; Newburyport, 9; Somerville, 6; Fall River, 17; Haverhill, 4; Holyoke, 11; Pittsfield, 2. Total, 342.

Prevalent Diseases.—Consumption, 70; pneumonia, 38; scarlet fever, 13.

GEORGE DERBY, M.D.,
Secretary of the State Board of Health.

DEATHS IN BOSTON for the week ending Saturday, May 2d, 156. Males, 73; females, 83. Accident, 2; apoplexy, 2; asthma, 1; aneurism, 1; inflammation of the bowels, 1; disease of the bladder, 1; bronchitis, 10; inflammation of the brain, 1; disease of the brain, 3; burned, 1; cancer, 2; cholera infantum, 1; cholera morbus, 1; consumption, 29; convulsions, 6; debility, 6; diabetes, 1; dropsy, 1; dropsy of the brain, 5; drowned, 1; dysentery, 1; erysipelas, 2; scarlet fever, 9; typhoid fever, 2; disease of the heart, 8; disease of the hip, 1; disease of the kidneys, 2; disease of the liver, 1; congestion of the lungs, 1; inflammation of the lungs, 16; marasmus, 7; malformation, 1; old age, 7; paralysis, 3; premature birth, 4; puerperal disease, 3; rheumatism, 1; scrofula, 1; syphilis, 1; whooping cough, 5; inflammation of the uterus, 1; unknown, 3.

Under 5 years of age, 60; between 5 and 20 years, 14; between 20 and 40 years, 29; between 40 and 60 years, 21; over 60 years, 32. Born in the United States, 107; Ireland, 34; other places, 15.

Supplement to the
Boston Medical and Surgical Journal.

THE PRIZE ESSAYS OF THE

BOYLSTON MEDICAL SOCIETY,

FOR 1874.

[The experiments in both the following Essays were performed in the laboratory of Dr. H. P. Bowditch, Professor of Physiology in the Harvard Medical School.]

EXPERIMENTS ON THE ACTION OF BILE IN PROMOTING THE ABSORPTION OF FATS.

By Charles H. Williams, A.B., of Boston.

The Essay to which was awarded the First Prize of the Boylston Medical Society for 1874.

A number of experiments have been made, to prove that the presence of bile in the intestines favors the absorption of fat. Bidder and Schmidt* "fed a healthy dog with a carefully-ascertained amount of fat. By examination of its fæces, it was found to have absorbed 11.1 grammes for each kilogramme of its weight. Another dog, in which biliary fistula had been established, was similarly fed; the animal was allowed to lick some of the bile from the wound, and it was proved that but 2.24 grammes of fat had been absorbed for each kilogramme of weight. In a third, in which all access of bile to the intestine had been cut off, only 1.56 grammes was absorbed." Brodie† states that, after tying the common bile duct on young cats, no chyle was to be found in the lacteals and thoracic duct. Tiedmann and Gmelin‡ find that the fat absorption is diminished, but not entirely stopped under these circumstances. Ludwig§ says: "The proof of the diminished absorption of fat during the obstruction to the inflow of bile is shown by this, that the chyle coming from the small intestine is poorer in fat, and the fæces are correspondingly rich in fat." Kühne‖ gives an experiment, in which a healthy dog was carefully fed with fat, until it began to appear in the fæces. The quantity which he was taking was then noted, a biliary fistula was made, and it was then found that the quantity of fat which could be given without having any appear in the fæces was very much smaller than before.

Such, then, being the established fact, how is it to be explained? Marshall¶ says: "It certainly does not appear to act chemically, by decomposing, or dissolving neutral fats, nor does it make, with oily matters, a permanent emulsion." The object of this paper, however, not being to discuss any chemical relation of the bile, we leave the point with this statement.

Another way in which bile may aid in the passage of fats into the lacteals, is by a mechanical influence of some kind. Wistinghausen** states, in beginning an account of some experiments on this subject: "The first (bile), attracted more strongly by the membranes, carries, mechanically, the particles of oil adhering to it, without having previously changed their chemical constitution." These experiments seem to have been the fullest and most complete which have yet been made on this subject. His apparatus and method was this:†† "A glass tube, six feet long, was bent so as to have one part shorter than the other; over the short (leg) was stretched a freshly-dried, thin mucous membrane, from an ox intestine. Through the long leg, the tube was filled with the liquid in question, the air removed by proper

* Marshall's Outlines of Physiology, p. 557.
† Quarterly Journal of Science and the Arts, January, 1823, p. 341.
‡ Verdauung, ii. p. 47.
§ Lehrbuch der Physiologie, ii. p. 658.
‖ Lehrbuch der physiologischen Chemie.
¶ Marshall's Outlines of Physiology, p. 556.
** Reichert und Du Bois Reymond's Archiv, 1873, No. 2, p. 130.
†† Ibid., p. 141.

means, and mercury, employed to obtain pressure, was poured into the long leg, until the fluid began to pass through the membrane on the short leg; then the height of the mercury was measured." In some later experiments, a larger tube was fitted round the end of the short leg, to form a cell, in which liquid could be placed.

Being led to make some experiments to illustrate the same points, the author desired to repeat the experiments of Wistinghausen; but as his methods were not fully explained, it was impossible to do so. For instance, he gives a number of experiments, in which oil came through a membrane with so many m.m. of mercury pressure; but he does not state whether the time was the same in each case, or not. Again, it is not stated in what way the mercury was poured into the long leg; whether in a constant stream, until the oil appeared through the membrane, or whether, after pouring in a certain amount of mercury, he waited, and then added more, and so on. No mention of time is made, in connection with his experiments, until we reach the ninth page of his essay, when we begin to have given the time required for the passage of the oil, with a certain pressure of mercury.

The following apparatus was, therefore, constructed. Over the lower ends of two glass tubes, parchment was stretched. The upper ends were connected with a pressure bottle, arranged in the following way: Into a Wolf bottle, was poured some mercury; through one neck was fitted a tube, which dipped beneath the mercury; the tubes were connected with another neck, and, through the third opening, water was forced in, thus compressing the air above, the pressure being shown by the rise of the mercury in the tube. When the required height was reached, the water was allowed to dribble in just fast enough to make up for leakage and the outflow through the tubes.

The object, then, was to note the difference in time, required to force oil through a membrane moistened with bile, and one wet with water, the pressure remaining the same. For this purpose, one piece of parchment was moistened with bile, the other with water; these tied securely over the ends of the tubes, oil poured into each tube, and pressure applied. After making eleven experiments with parchment, parchment-paper and hog's bladder, it was found that the results were not entirely consistent, that the different pieces of membrane were apt to vary in texture, and that it was difficult to prevent leakage. A membrane was then sought for which should be entirely homogeneous, and an apparatus which would admit of ready adjustment, and prevent leakage. The following apparatus was then constructed. A piece of brass tube, about a foot long, was bent to a U shape, one arm being longer than the other. To the top of the shorter arm, was soldered a brass plate, having a hole in the centre; upon this was placed a similar plate; the two were then ground to fit, and held together by four screws. The whole was held by a suitable stand, so that the plates were horizontal. To the longer arm, a pipe was tightly fitted, which connected the tube with a pressure apparatus, consisting of the ordinary arrangement of two bottles, the mercury in that which is placed on the higher level, running into the other, and compressing the air in it. The amount of pressure was indicated by a manometer tube.

In using this apparatus, the following order was followed. The

brass tube was first filled with the oil, or emulsion, which it was desired to pass through the membrane, until the oil was on a level with the lower brass plate. Next, the membrane, dry, or previously soaked in some fluid, was laid on the brass plate, no air coming between the membrane and the oil. The second plate was then put on and screwed down; and if the membrane had been previously wet with any fluid, a few drops of that fluid were placed upon it, to make sure that it should be kept moist. Next, the pressure apparatus was connected with the brass tube, clamped on, and the bottle of mercury raised until the manometer tube showed the requisite pressure. The time was then noted. After this, the membrane was watched with a Hartnack microscope (objective 2, eye-piece 3), placed vertically over it, until the drops of oil were seen coming through, when the time was again noted. The difference between the times, gave the minutes and seconds required for the oil to penetrate the membrane.

In the first experiments, parchment paper was used as the membrane. The small squares were all from the same sheet of paper. The following results were obtained:

I.—*Experiments with dry Parchment Paper.*—Pure olive oil was placed in the tube, and, in the first series, *no pressure* was used, the time being noted when the first glistening lines of oil appeared on the paper. The numbers indicate the minutes and seconds required for oil to come through the membrane.

First Series.—Time, 7′,30″; 11′; 11′,15″; 6′,45″; 10′; 10′; 12′,15″; 8′,15″; 11′. Temperature, 65° Fahr.

Second Series.—Seven and a half centimetres of mercury pressure. Time, 3′,15″; 3′; 3′; 4′; 3′; 2′,30″; 4′; 2′,45″; 3′,15″. Temperature, 70° Fahr.

Third Series.—Same pressure (seven and a half centimetres); temperature, 60° Fahr. Time, 6′; 5′,30″; 5′; 6′; 4′,45″; 5′; 5′,15″; 6′,30″.

Fourth Series.—Fifteen centimetres of pressure. Temperature, 68°. Time, 3′; 3′,45″; 3′; 3′,30″; 4′; 2′,30″; 4′,25″; 3′,30″; 3′,15″; 3′; 4′,45″.

Fifth Series.—Thirty centimetres of pressure. Temperature, 64°. Time, 2′,57″; 3′,10″; 3′; 2′; 2′; 2′,50″; 2′,10″; 2′,10″; 2′,55″.

II.—*Experiments with Parchment Paper Wet with Water.*

First Series.—Fifteen centimetres of pressure. Temperature, 68°. Time, 14′,20″; 12′; 12′; 17′; 11′.

Second Series.—Thirty centimetres of pressure. Temperature, 67°. Time, 4′,30; 6′,10″; 4′,20″; 8′, 20″; 6′; 6′; 3′,50″; 5′,45″; 3′30″; 5′,45″.

III.—*Experiments with Parchment Paper Wet with Bile.*

First Series.—Fifteen centimetres of pressure. Temperature, 64°. Time, 4′; 4′,20″; 4′,45″; 3′,55″; 3′,45″; 3′,05″.

Second Series.—Thirty centimetres of pressure. Temperature, 66°. Time, 1′,45″; 2′,00; 1′,48″; 1′; 1′,05″; .45″; 1′.

Table A.—Parchment Paper used as the Membrane.

Pressure of Mercury in c. m.	Membrane Dry.			Wet with Water.			Wet with Bile.		
	No. of Exp.	Average.	Variation.	No. of Exp	Average.	Variation.	No. of Exp.	Average.	Variation.
0	9	9′,46″	5′,30″						
7½	17	4′,17″	4′,						
15	11	3′,31″	2′,15″	5	13′,16″	6′,	6	3′,58″	1′,40″
30	9	2′,35″	1′,10″	10	5′,25″	4′,50″	7	1′,20″	1′,15″

(NOTE.—When the membrane was used wet, the surfaces were wiped of superfluous fluid, just before placing the membrane on the oil.)

From these experiments, it will be seen that, with each pressure, oil came through a membrane moistened with bile faster than when it was moistened with water, though not so fast as when dry. There was found, however, to be a great variation between the different experiments at the same pressure, and at times the oil would well up rapidly through some thin spot of the paper, before it had appeared on any other part. Also, on removing the oil from some of the pieces, by soaking in ether, drying, and repeating the experiments, the results were very unsatisfactory.

A new membrane was therefore sought for, which should be as homogeneous as possible, and sufficiently porous to admit of oil passing through without much difficulty. As the membranes through which absorption takes place must be supposed to contain minute, capillary spaces, through which the fats find their way, an endeavor was made to construct an artificial membrane or substance, which should consist mostly of minute capillary spaces. For this purpose, sheets were made of plaster of Paris, the thickness being reduced until one millimetre was reached. They were prepared by pressing out this fluid plaster between pieces of thick plate glass, separated from each other one millimetre. When quite firm, the plaster was removed, and, while translucent, all portions containing air-bubbles were marked; the cake was then dried, and cut into squares for use. Twenty experiments were then made with different sheets of dry plaster, the oil having a pressure of fifteen centimetres of mercury.

It was now found, however, that, to obtain uniform results, the plaster must always be made with the same proportion of water; otherwise, when more water was used, the pores were rendered larger, and the oil came through more readily. The following experiments were all made with squares from one sheet of plaster. Thin plaster was used as the membrane (Sheet I.), and pure olive oil was placed in the brass tube. In the first series of experiments, with no pressure, the capillary force was not sufficient to bring any amount of oil to the upper surface, so the time was taken when the first glistening lines appeared.

IV.—*Experiments with Dry Plaster.*

First Series.—No pressure. Temperature, 62°. Time, 5′,50″; 6′, 30″; 6′,15″; 6′,15″.

Second Series.—Seven and a half centimetres pressure. Temperature, 54°. Time, 4′; 3′,50″; 4′,10″; 3′,45″.

Third Series.—Pressure, fifteen centimetres. Temperature, 62°. Time, 3′,20″; 3′,10″; 3′,30″; 3′,05″; 3′,15″.

Fourth Series.—Pressure, thirty centimetres. Temperature, 62°. Time, 1′,50″; 1′,52″; 1′,54″; 1′,32″.

V.—*Experiments with Plaster Wet with Water.*

Pressure, thirty centimetres. Temperature, 62°. No oil in 40 minutes; no oil in 45 minutes. After two hours and three quarters, a few drops appeared.

VI.—*Experiments with Plaster Wet with Bile.*

Pressure, thirty centimetres. Temperature, 64°. Time, 10′,30″; 8′; 9′,25″; 8′,45″; 10′,20″.

Table B.—Thin Plaster (Sheet I.) used as the Membrane.

Pressure of Mercury in c. m.	Membrane Dry.			Wet with Water.			Wet with Bile.		
	No. of Exp.	Average.	Variation.	No. of Exp.	Average.	Variation.	No. of Exp.	Average.	Variation.
0	4	6′,12″	0′,40″						
7½	4	3′,56″	0′,25″						
15	5	3′,16″	0′,25″	No oil in 40 minutes.					
30	4	1′,47″	0′,22″	" " 45 "					
				3, few drops after 2½ hrs.			5	9′,24″	2′,30″

The next series of experiments were made with another sheet of plaster (Sheet II.).

VII.—*Experiments with Dry Plaster.*

First Series.—Pressure, fifteen centimetres. Temperature, 64°. Time, 3′,11″; 3′; 3′; 2′,35″; 2′,45″; 2′,55″.

Second Series.—Pressure, thirty centimetres. Temperature, 64°. Time, 2′,14″; 2′,10″; 1′,56″; 2′,02″; 2′,04″; 2′.

VIII.—*Experiments with Plaster Wet with Water.*

First Series.—Pressure, seven and a half centimetres. No oil after twelve hours.

Second Series.—Pressure, fifteen centimetres. No oil after 6 hours. After 23 hours, a large amount.

Third Series.—Pressure, thirty centimetres. After 3½ hours, there was a thin layer over the plaster. After 4½ hours, a small amount had come through.

Fourth Series.—(a) Pressure, forty-five centimetres. After one hour and five minutes, one drop appeared on the surface of the plaster; at one hour and thirty-five minutes, about twelve drops had come through; in two hours, quite a number of drops.

(b) After one hour and ten minutes, one drop; one hour and twenty minutes, a number of drops; one hour and fifty minutes, the surface of the plaster was thickly dotted over with them.

Fifth Series.—Pressure of sixty centimetres. Time, 1 hour and 2 minutes; 1 hour; 50 minutes.

IX.—*Experiments with Plaster Wet with Bile.*

First Series.—Pressure, fifteen centimetres. After 2 hours, a little oil appeared. After 2¼ hours, a little oil appeared.

Second Series.—Pressure, thirty centimetres. Time, 20′; 19′,20″; 20′,10″; 19′,30″; 19′,18″; 20′; 20′; 19′,30″; 19′,20″.

Third Series.—Pressure, forty-five centimetres. Time, 13′; 12′,20″; 14′,30″; 15′,30″

Fourth Series.—Pressure, sixty centimetres. Time, 8′; 7′; 7′;

Table C.—Thin Plaster (Sheet II.) used as the Membrane.

Pressure of Mercury in c. m.	Membrane Dry.			Memb. Wet with Water.			Memb. Wet with Bile.		
	No. of Exp.	Average.	Variation.	No. of Exp.	Average.	Variation.	No. of Exp.	Average.	Variation.
7½				1; no oil after 12 hours.					
15	6	2′,54″	0′,36″	No oil after 6 hours; after 23 hours, large amount.			After 2 h., a little oil. " 2¼ " " " "		
30	6	2′,04″	0′,18″	3½ hours, thin layer. 4½ " small amount.			9	19′,41″	0′,52″
45				1 h. 50′, number of drops. 2 hours, " " "			4	13′,50″	3′,10″
60				2 | 57′ | 12′			3	7′,20″	1′

In these experiments, the bile was taken from an ox, killed only a few hours before; it was neutral to test paper, and had a specific gravity of 1023.

In all cases in which the membrane was moistened with bile, when the oil began to come through, it appeared in very finely-divided particles, just visible by the microscope, giving in mass the appearance of a whitish cloud rising from the surface of the plaster.

On the other hand, when the membrane was moistened with water, the oil came through, forming a small globule, which gradually enlarged. Finally, when it had reached sufficient size for its specific gravity to free it from its adhesion to the plaster, it rose to the top of the water, which covered the membrane.

All the previous experiments were made with olive oil; a few were now tried with different emulsions, but with uncertain results. The oil seemed to come through first, and when the emulsions were colored, most of the coloring matter was retained on the under surface of the plaster.

Rabbit's bladder was also tried for the membrane; but this could not be procured in sufficient quantity to give any extended series of experiments.

What, then, can we infer from these results?

1. That when a membrane is wet with bile, it allows oil to pass through more easily than when it is wet with water.

2. That, as the pores of the plaster cannot be supposed to change their shape, being made of a perfectly inelastic material, therefore bile cannot favor the passage of the oil by affecting any such change of form in the pores.

3. That, as the oil is more finely divided when it has passed through plaster moistened with bile than when it is wet with water, or used dry, therefore the bile must have some influence in effecting this.

Ludwig* makes the following statements, which have some bearing on these facts: "The assistance which the bile renders to the passage of fat is best explained by this, that the obstruction which the pores oppose to the passage of fats is diminished.

"This (conclusion) can be reached in various ways; for example (a), by this, that the bile, pressed into the thin, mucous membrane, alters the form of the pores, and lessens the firmness of the tissues; or (b), that it makes the surface of the pores more slippery, so that the friction between the oil and the walls of the pores is lessened; or (c), that it makes the fat drops more capable of adapting themselves to the form of the porous canals, in that it diminishes the so-called drop-tension of the fats." The first of these explanations we have eliminated by the use of plaster as the membrane; the third seems likely, on account of the finely-divided appearance of the fat which comes through bile; and the second, although we have no proof of it, seems probable.

It may be said that, although the previous experiments show that oil goes through faster when the membrane is wet with bile than when it is moistened with water, yet that this does not prove that if some other liquid were used it would not go through faster still. The following experiments were performed to examine this point.

* Lehrbuch der Physiologie, ii. page 658.

The whole series was made from pieces of the same sheet of plaster (Sheet III.).

X.—*Experiments with Dry Plaster* (made to be able to compare the times which this sheet of plaster gives with the times of the other sheets).

First Series.—Pressure, fifteen centimetres. Temperature, 65°. Time, 3′,35″; 3′,45″; 3′,15″.

Second Series.—Pressure, thirty centimetres. Temperature, 65°. Time, 1′,40″; 1′,55″; 2′; 1′,55″.

Third Series.—Pressure, forty-five centimetres. Temperature, 62°. Time, 1′,25″. 1′,50″; 1′,25″; 1′,45″; 1′,23″.

Fourth Series.—Pressure, sixty centimetres. Temperature, 62°. Time, 1′,05″; 1′; 1′,10″; 1′,07″.

XI.—*Experiments with Plaster wet with Bile which had been made Alkaline with a Solution of Soda.*

Pressure, sixty centimetres. Temperature, 64°. Time, 3′,50″; 3′, 52″; 3′,30″.

XII.—*Experiments with Plaster wet with a one per cent. Solution of Soda.*

Pressure, sixty centimetres. Temperature, 60°. Time, 3′,20″; 4′; 3′,45″; 4′,30″; 4′.

XIII.—*Experiments with Plaster wet with fresh Ox Bile.*

Pressure, sixty centimetres. Temperature, 65°. Time, 4′,40″; 4′, 50″; 5′,10″.

XIV.—*Experiments with Plaster wet with Bile made Acid by Hydrochloric Acid.*

Pressure, sixty centimetres. Temperature, 57°. Time, 5′,10″; 5′, 45″; 5′,15″.

XV.—*Experiments with Plaster wet with Bile made Acid by Acetic Acid.*

Pressure, sixty centimetres. Temperature, 57°. Time, 6′,30″; 5′, 42″; 5′,20″; 6′.

XVI.—*Experiments with Plaster wet with Water.*

Pressure, sixty centimetres. Temperature, 65°. Time, 6′,40″; 6′, 15″; 6′,30″.

XVII.—*Experiments with Plaster wet with Hydrochloric Acid (one per cent.).*

Pressure, sixty centimetres. Temperature, 65°. Time, 9′; 8′,45″; 9′,30″.

Table D.—Thin Plaster (Sheet III.) used as the Membrane.

Pressure of Mercury in c. m.	Membrane Dry.			Memb. Wet with Water.			Memb. Wet with Bile.		
	No. of Exp.	Average.	Variation.	No. of Exp.	Average.	Variation.	No. of Exp.	Average.	Variation.
15	3	3′,32″	0′,30″						
30	4	1′,53″	0′,20″						
45	5	1′,33″	0′,27″						
60	4	1′,05″	0′,10″	3	6′,28″	0′,25″	3	4′,53″	0′,30″
	Wet with Soda Solution (1 per cent.)			Wet with hydrochloric acid (1 per cent.)			Wet with bile made alkaline with soda solution.		
60	5	3′,55″	1′,10″	3	9′,05″	0′.45″	3	3′,44″	0′,22″
	Wet with bile made acid by hydrochloric acid.			Wet with bile made acid with acetic acid.					
60	3	5′,23″	0′,35″	4	5′,53″	1′,10″			

Or, arranging them in the order of their time, we get:—

Oil through dry plaster (60 centimetres pressure),									1′,05′
"	"	plaster wet with alkaline bile (60 centimetres pressure),							3′,44″
"	"	"	"	"	solution of soda (1 pr. ct.) (60 centimetres pressure),				3′,55″
"	"	"	"	"	bile	"	"	"	4′,53″
"	"	"	"	"	hydrochloric acid and bile	"	"	"	5′,23″
"	"	"	"	"	acetic acid and bile	"	"	"	5′,53″
"	"	"	"	"	water	"	"	"	6′,28″
"	"	"	"	"	hydrochloric acid (1 pr. ct.)	"	"	"	9′,05″

From these experiments we find that when the membrane is moistened with an alkaline fluid, fats go through quicker than with bile, water or acid; and that the addition of alkali to the bile allows the fat to pass in the shortest time of all. In the small intestine, we have two alkaline fluids poured out, the pancreatic and intestinal juices; these soon render the chyme, which comes down from above, alkaline. Now, if it is allowable to argue from results obtained from inorganic capillary tubes, what the action would be in organic pores, it would seem to follow, from the previous experiments:—

First, that, when the bile is mixed with the alkaline fluids of the intestine, its power of promoting fat absorption will be greatest.

Secondly, that, if either the bile or the alkaline fluids be prevented from entering the intestine, still a certain amount of fat absorption will take place; for if the bile alone flows in, it will exert its specific influence in aiding the mechanical passage of the fats, and if the alkaline fluids only enter, considerable fat will still penetrate the walls, aided by the alkalinity of the fluid, even if there were no other action. These suggestions seem to be sustained by previous experiments, for it has been found that, when all bile was cut off from the intestine, still some fat was absorbed; and, also, when a pancreatic fistula was made, considerable fat was still found in the lacteals—more in amount in the last case, perhaps, because there was still the alkaline intestinal juice, which could not be cut off.

To conclude, we have endeavored to show, by new experiments, and methods differing somewhat from those which have been tried before:—

1. That the passage of neutral fats, through capillary canals or pores, is favored by the presence of bile in those pores.

2. That this action is increased when the bile is rendered alkaline, and diminished when it is acid.

3. That the action cannot be due to the bile changing the form of the pores.

4. That, after passing through membranes moistened with bile, the fats appear more finely divided than with membranes wet with other substances, apparently showing that the drop-tension or cohesion of the fat has been affected.

INTESTINAL DIGESTION.

By G. M. GARLAND, A.B., of Lawrence.

The Essay to which the Second Prize of the Boylston Medical Society was awarded.

ALTHOUGH those who are familiar with the processes of digestion admit that the intestinal juices are important auxiliaries to the performance of that function, yet I venture that the ideas of many will be startled by the recently advanced opinions of Bernard, who states that when the food has escaped from the stomach and become impregnated with the bile, digestion has yet to begin. At this stage of the proceedings, a greater part of the aliments remain unaltered. A small portion of the albuminoids only have been transformed, but the first result of the contact of the bile is to annul that transformation and to precipitate a part of the peptones already formed. "It is, then, in an insoluble form," he says, "that the aliments present themselves before the agents of the intestines which must digest them really and definitely."

This opinion of Bernard is based upon a series of complicated experiments, undertaken by many physiologists with the view of differentiating the functions peculiar to the several secretions of the alimentary canal. Among the intestinal agents, Bernard includes the pancreatic juice as well as the intestinal secretions proper, for he says that it is by the intervention of the pancreatic juice that digestion really begins. I desire, in this paper, however, to illustrate some of the experiments which have been made upon the pure intestinal juice, from which it will be seen that a great diversity of opinion exists among different observers as to the solvent power of that secretion upon the various types of food.

There exist, in the intestinal mucous membrane, four sets of glands, viz.: the solitary glands, Peyer's patches, Brunner's glands and Lieberkühn's follicles. Peyer's patches are merely small colonies of the solitary glands or vesicles, and are ordinarily closed, though they are sometimes found open in the pig (Krause), and also in the human intestine (Allen Thompson). Kühne states that they do not appear to contribute anything to the intestines, except under pathological conditions; but Bernard says they secrete mucus. On the other hand, Brücke has demonstrated that they are small lymph glands, and, hence, have no participation in the performance of digestion.

Brunner's glands are peculiar to the duodenum, and were formerly supposed to correspond to the pancreas in secretion as well as in structure. From recent experiments, however, Bernard concludes that the secretion of these glands possesses no other chemical properties than those of mucus. He says they are analogous to certain agglomerations of muciparous glandules found in the pharynx of the horse, and that their only function is to facilitate the impregnation of the chyme by the bile and pancreatic juice. Lieberkühn's follicles are found throughout the entire length of the intestine, and to them is attributed the function of secreting the intestinal juice proper. It is worthy of remark, however, that Schiff observed that the fluid obtained from a fistula which was located in the duodenum gave more positive digestive results than if the fistula were established lower down in the ileum. From this, we may infer either that Brunner's glands are an

auxiliary in the one case, or that the strength of the secretion of Lieberkühn's glands diminishes as we recede from the duodenum.

The difficulty of determining the function which pure intestinal juice serves in the process of digestion may be understood only when one has considered the difficulty of eliminating from the problem all the extraneous contributions of neighboring organs and of reducing to a minimum the inevitable derangement of the functional integrity of the abdominal organs which follow mechanical interference. So important is this latter factor alone, that Hoppe-Seyler, in apparent despair, says that until we can positively establish the normal condition of the secretion of the intestinal mucous membrane, all experiments must remain without trustworthy results.

Among the early investigators of the subject, Frerichs endeavored to obtain the action of the pure secretion by including a loop of the intestine between two ligatures, washing it out carefully, and then introducing food for digestion. Zander passed a piece of cork into the duodenum below the pancreas, and then tied a ligature about both intestine and cork. Other experimenters adopted similar manœuvres, but their results are of value only as demonstrating the futility of the means they employed for eliminating the secretion of the organs above. By the showing of Schmidt and Zander themselves, the analysis of the juice which they obtained discovered an obvious proportion of gallic acid salts and other imported ingredients.

In 1858, Prof. W. Busch was so fortunate as to have under his observation a woman who had been gored by a bull in such a manner that two fistulæ were established in the small intestine. The right fistula communicated with the stomach by a short section of the intestine, while the second opening, removed a few inches to the left, was connected with the anus by the major part of the intestine. As a result of the inflammation consequent upon the injury, one end of the bit of intestine between the two orifices became sealed up so as to form a cul de sac, and thereby render impossible any communication between the upper and lower portions of the intestine. All the ingesta, therefore, escaped from the right fistula, which constituted an artificial anus. Owing to the small amount of intestine available to the woman, she emaciated with terrible rapidity, and was possessed of so ravenous an appetite that she would continue eating while the food was pouring out of the hole on the right. Busch experimented by introducing free food into the left fistula and allowing it to escape naturally, or by enclosing it in a net bag that he might be able to withdraw it again. He says, in substance, that the amount of the intestinal secretion was only sufficient to moisten the intestinal walls. It changed starch into sugar, and converted cane sugar into grape sugar. It decomposed protein bodies, but with the appearance of putridity and odor of ammonia. The bits of meat which he introduced free into the fistula were entirely dissolved before reaching the anus, though the meat held in the bag was not so completely decomposed. The secretion did not apparently affect fats, as the latter were unabsorbed unless mixed with gall.

Busch did not find the temperature of the abdominal viscera increased during digestion. Braune, however, who had a somewhat similar case of fistula under his charge, noticed an increase of four-tenths of a degree in the temperature of the intestines during digestion. Both

Busch and Braune agree that the secreting glands of the intestine have alternate periods of activity and repose.

The first real advance was made in this investigation by Thiry, when he succeeded in isolating a portion of the small intestine. The method he pursued was as follows: He took a dog which had been fasting at least twenty-four hours, and opened its abdominal cavity by an incision about two inches in length along the linea alba and below the umbilicus. Drawing out therefrom a loop of the intestine, he excised from it a piece ten to fifteen centimetres long, taking care not to sever the piece from the mesentery, or any of its vessels and nerves. Turning aside the isolated piece, he reëstablished the continuity of the intestinal tract by bringing together with sutures the cut ends of the part above and the part below, and then replaced the intestine, minus fifteen centimetres, in the abdomen. Taking up again the isolated piece, he closed one end of it by sutures, and thus made a blind sac of it. He then passed the blind end of the loop into the abdomen, and stitched the open end or mouth of the bag to the edges of the incision in the abdominal walls. If the animal did not die of peritonitis in a few days, he says, the wound generally healed in fourteen days, leaving only a fistulous opening, from which the intestinal juice could be collected.

Thiry experienced a serious obstacle to the establishment of a permanent fistula from the liability of the isolated piece to prolapse. This prolapse occurred in two ways. Either the inner end would become introverted, and, finally emerge from the outer opening, followed by the whole length of the piece, or the outer end would become everted, as in case of prolapsus recti. To avoid these casualties, he made a "bias," so to speak, in the outer end of the loop, which contracted the orifice sufficiently to retain the parts in place, but had the disadvantage of preventing the free egress of the secretion or the ready introduction of food or sponges.

After the healing of the wound, he observed that the fistula did not ordinarily secrete much, unless the mucous membranes were irritated, mechanically, by the insertion of sponges, catheters, or weak hydrochloric acid, or by the application of the induced current. By the assistance of such irritants, he obtained from his fistula four grammes of secretion in an hour. At this rate, a dog would secrete, from the entire length of his intestine, about three hundred and sixty grammes of juice during the five hours allowed him for the digestion of his dinner. The irritation of the vagus nerve by electricity occasioned no secretion, and he inferred that the action of Lieberkühn's glands was governed by the sympathetic system.

Thiry describes the secretion which he obtained as a thin, clear, yellow fluid, strongly alkaline, with an average specific gravity of 1.0115. It gave a precipitate of albumen on being acidified and boiled. An analysis of 1,000 parts of the juice gave: water, 975.85; inorganic salts, 8.79; albuminates, 8.02; other organic materials, 7.34.

As regards the digestive virtues of the pure secretion, Thiry's experiments were almost negative. He says that fats, starch, coagulated white of egg, red muscle and glue were not digested, either within or without the fistula. Fibrin was digested in the natural alkaline solution, but remained unaffected if the juice were acidified or even neutralized. In order to assure himself as to the structural integrity of the isolated piece, he made microscopic examinations after the death

of the animals, but could detect no difference between the structure of the fistula and of other parts of the intestine contiguous to it. He concluded, therefore, that to be of any value, the intestinal secretion must be associated with the other digestive fluids.

As a point of collateral interest, it may not be amiss to mention here the influence of certain drugs upon the isolated loop. Thiry gave the dog at one time a strong dose of sulphate of magnesia, and again a pill of pulverized senna leaves. Both doses produced active catharsis, but did not affect the isolated piece. An injection of a solution of either of the drugs into the loop itself produced no more effect than an injection of distilled water. Six drops of croton oil rubbed upon the dog's abdomen produced vomiting and purging, but had no influence upon the fistula. He reasons, therefore, that these drugs produce catharsis by so exciting peristaltic action that the intestines are emptied of their contents before time has been allowed for the customary reäbsorption of the fluids of the chyme.

Dr. W. Leube reviewed Thiry's experiments, and agreed with the latter that the intestinal juice does not convert starch into sugar. He succeeded, however, in changing cane sugar into grape sugar, and, also, induced an acid fermentation. This fermentation, however, he attributed to the presence of vibriones, as it only occurred when these animalcules were present. In this connection, however, it will be remembered that Bastian asserts that vibriones are one of the products of, rather than the cause of, fermentation.

Leube says that raw fibrin dissolves slowly, and without swelling. He conducted his fibrin experiments by subjecting a couple of shreds of fibrin to the action of five to ten centimetres of intestinal juice for ten to sixteen hours in a digesting oven. The solution was then diluted to double its volume, heated to the boiling point, and acidified with acetic acid to precipitate the albumen. When no more precipitate was formed on further addition of acid, he filtered the fluid and tested the filtrate for peptones. The following are the principles upon which he relied for proving the absence or presence of peptones:

A. The peptones are not precipitated by heating.

B. A solution of peptones, heated with nitric acid in excess, assumes a yellow color, which becomes still deeper on the addition of ammonia (xanthroprotein test).

C. If a solution of the protonitrate of mercury be combined with a solution of peptones and boiled, and then a few drops of nitrous or nitric acid be added, a dark red color will result (Millon's reaction).

D. If the peptone solution be mixed with soda, and a few drops of sulphate of copper, it will form a rose-red color, which gradually passes into a violet color on the further addition of sulphate of copper. If the amount of peptones is considerable, the play of colors will take place more slowly, and requires more of the copper salt.

E. If the ferrocyanide of potash be added to the peptone solution, it produces a turbidity which amounts to a precipitate on heating.

F. Sesquichloride of iron causes a whitish yellow precipitate, which dissolves in an excess of the reagent.

Leube states that, measured by these tests, no other albuminoid substance than fibrin was digested.

Schiff declared that a fistula, to be successful, must be located in the duodenum. In one so situated, he succeeded in digesting fresh

casein, fibrin, and cooked or fresh meat. Starch was converted into sugar as readily as by the pancreatic juice, and oils were emulsified. In other parts of the small intestines, these results were more or less modified. He noticed an evident derangement in the circulation of the isolated part in one instance, and twice discovered atheromatous degeneration in the walls of the vessels of the fistulæ. He remarks that the intestinal villi persisted after the operation, and hence could not owe their existence to the influence of the gall or pancreatic juice.

Wittich rejected Thiry's method, and chose, in preference, a glycerine infusion of the mucous membrane of the intestine, which had been previously washed out by a stream of distilled water. He only experimented on two animals, a porpoise and a rabbit. In both cases, he obtained negative results with fibrin, and in only the first case did he succeed in converting starch into sugar.

In an interesting article upon certain ferments which change starch and cane sugar into grape sugar, Dr. Victor Paschutin relates his experience with intestinal juice. In making a fistula by his method, which is somewhat modified from Thiry's, the hair must be carefully removed from the abdomen of a dog, and an incision, three to five centimetres long, made in the linea alba, between the ensiform cartilage and the umbilicus. The duodenum may be easily recognized and drawn out, as it has no mesentery (Quain). Two ligatures should be passed around the duodenum, close together, and about two and a half centimetres below the pancreas. The intestine may then be severed between the ligatures, and the upper portion replaced in the abdomen.

Owing to the difficulty of following down the jejunum, Paschutin resorted to the expedient of grasping a loop of the intestine haphazard between his thumb and finger, and then injecting the portion above with water. By this means, he could easily follow up the distended gut from his fingers to the jejunum, which is readily recognized by its short mesentery. Here the process of ligaturing and cutting is repeated, and we have a loop consisting of the lower part of the duodenum and the jejunum. The ligatured ends of the upper part of the duodenum and lower part of the jejunum must then be cut away, and the continuity of the intestine restored by sutures. The inner end of the loop, which is everted by the contraction of its external muscular coat, must be inverted so as to approximate the edges of the serous coat, and stitched in order to form a cul de sac. The outer end is then stitched to the edges of the abdominal wound, which is closed up to the intestine. The minor details of the operation for the prevention of hæmorrhage or other casualties will be too obvious to require specification here. The advantage of Paschutin's fistula over that of Thiry's depends upon the fact that it is much longer, and that it is fixed by the short mesentery of the jejunum, which renders prolapse of the part impossible.

On account of greater convenience in collecting a large amount of digestive fluid, and of comparing experiments with different animals, and because the fistula is inapplicable to some animals, as to the young, for instance, Paschutin prepared water infusions of the intestinal mucous membrane in the following manner. Having bled an animal to death, he removed the intestines, and washed out all their contents by a stream of water admitted through the rectal extremity. As soon as the water came through clear, he grasped the intestine by its two ends,

and shook the contained water back and forth to disengage any particles still clinging to the walls. This was continued for about five minutes. He cautions against too long and too careful washing, as the active properties of the mucous membrane will be dissolved out also, as was the case with Wittich, who washed the rabbit's intestine for half an hour. The mucous membrane is then easily scraped from the muscular layers beneath, comminuted with shears, or rubbed up with ground glass, and mixed with three to six parts of water. After standing at the ordinary temperature from a quarter of an hour to two hours, it is filtered through linen, or through a tunnel containing ground glass, and then through paper. The fluid thus obtained is of a yellow color, alkaline reaction, and with an odor of the intestinal canal.

Paschutin does not state the mode of his experiments upon albuminoid bodies and fats, but gives, in minute detail, the processes which he adopted for testing the action of the above infusions upon starch. These processes are so complete and delicate that I desire to give them somewhat at length. His task was not merely to show that sugar appeared after starch and intestinal juice had been mixed and exposed to a certain temperature, but to prove that sugar was not due to microscopical organisms or the influence of the air, but was formed at the expense of the starch under the influence of the digestive fluid, and also that it was not the result of some change in the digestive fluid itself.

To demonstrate these points, he pours into three glasses (I., II., III.) equal amounts of the water infusions, about fifty centimetres, and heats glass I. for a short time at a temperature from 80° to 90° C. This temperature coagulates all the albumen, and, as will be seen later, destroys all diastatic virtues which the fluid may have possessed. To I. and II. he then adds equal amounts (twenty-five centimetres) of freshly prepared starch paste, and to glass III. an equal volume of water. A fourth glass receives, simply, a known amount of starch paste. Thus,

Glass I. contains intestinal juice (previously heated), 50 ccm. + starch paste, 25 ccm.

Glass II. contains intestinal juice (unheated), 50 ccm. + starch paste, 25 ccm.

Glass III. contains intestinal juice (unheated) 50 ccm. + water, 25 ccm.

Glass IV. contains only a certain amount of starch paste.

These glasses he then places in a water-bath, pendant through the openings of a floating disk, and keeps them at a temperature of 35° to 40° C. for a short time, and then interrogates them for their sugar contents. To this end he employs the following reactions:—

Moore's Reaction.—This test, as is well known, depends upon the fact that a colorless fluid, containing sugar in solution, by warming with a caustic alkali, will turn yellow, then red, brown, and, finally, black.

A. The play of color is more active as the degree of temperature to which the solution is subjected is higher.

B. If two portions of a sugar solution, equal in volume and concentration, be mixed with equal amounts of alkaline solutions of different degrees of density, a more active color-reaction consorts with the more concentrated alkaline solutions.

C. On the other hand, if the sugar solutions vary in concentration, and are mixed with equal amounts of one and the same alkaline solution, the reaction favors the higher per cent. sugar solutions.

D. If different amounts of a concentrated alkaline solution be added to equal volumes of a sugar solution, the color will be found to diminish with the larger amount of the alkali. The opposite effect takes place, however, if the alkaline solution be weak. (This latter statement is true only within certain limits.)

Hence, it follows from (C) that if equal volumes of similar fluids which contain different amounts of sugar be combined with similar amounts of an alkaline solution of one and the same concentration, and be then heated at one and the same temperature for the same length of time, we can quantitatively determine the amount of sugar contained in each fluid by comparing the rapidity with which the color-changes appear and follow each other.

As the value of this test depends upon the accurate comparison of depths of color and color-changes, it is obvious that the test glasses must be colorless, of the same diameter, and must possess the same ability to transmit heat. Accordingly, Paschutin took a dozen test-tubes, of medium size, and poured into each one an equal amount of sugar solution and of caustic soda solution, and chose those tubes in which the fluids stood at the same level. These tubes were then corked to prevent access of carbonic acid from the air, placed in the apertures of a wooden disk, and submitted in a water-bath to a temperature of 80° to 90° C. Only those tubes were approved in which the colors appeared and changed simultaneously. The water-bath should be made of glass, for convenience in observing the play of colors.

To determine the delicacy of Moore's reaction in a mixture composed of starch, a ferment and an alkaline hydrate, Paschutin prepared a compound consisting of a four per cent. solution of starch (1 part), a ten per cent. solution of caustic soda ($\frac{1}{2}$ part), and human saliva filtered and diluted ten times (1 part). As the alkaline hydrate was added to the starch before the ferment, no diastatic action could result from the contact of the two latter. From this mixture, he poured ten ccm. into each one of ten approved test tubes, and then added to each one, respectively, 0, 1, 2, 3, &c. drops of a sugar solution. (0.25 grammes of sugar to 100 ccm. of water; one drop $= \frac{1}{25}$ ccm.) The tubes were then well shaken, corked, and placed for three to five minutes in hot water (85° C.) until the colors were distinct. With care, he says, one may distinguish the color from one drop of the sugar solution. With two drops, the color is well defined; and with three drops it cannot escape the most careless observer. One drop of the above sugar solution, however, contains 0.0001 gramme of sugar, which, in relation to the whole amount in each tube (10 ccm.), equals 0.001 per cent, If, now, instead of a 0.25 per cent. solution of sugar, one employs a 0.1 per cent. solution, no difference from the above result can be distinguished. A one per cent. solution gives a scarcely traceable difference. It follows, therefore, that if ten ccm. of the mixture contain 0.01 gramme of sugar (0.1 per cent.), the smallest difference in the variation of the sugar contents which is distinguishable by Moore's test is equal to about 0.0004 gramme (0.004 per cent.) Generally, so long as the

sugar contents do not exceed 0.5 per cent. the color confines itself exclusively to the yellow tint.

Having thus illustrated the great delicacy of Moore's reaction, and having obtained a number of approved test tubes, Paschutin returns to the mixtures which he left floating in the water-bath (page 16). If diastatic action be possible under any of the conditions given in any of the four glasses, it must have taken place by this time. Accordingly, he removes the disk from the bath, and subjects II. and III. to a high temperature, to coagulate the albumen, and then filters them together with I., which was previously heated. This precaution is merely a matter of convenience, as the reaction shows no essential difference if the albumen is not removed, but requires much more time.

He next arranges a great number of approved tubes in four rows, supported by a wooden disk floating in ice cold water. Each row of tubes corresponds to one of the solutions above described, and into each tube is poured five ccm. of the mixture corresponding to its row. To each tube of the four rows is then added two ccm. of a six per cent. to ten per cent. solution of caustic soda. The tubes are then corked and transferred with the disk into a hot bath, where the color-changes can occur if sugar be present.

Besides Moore's reaction, Paschutin employed Trommer's test with Fehling's solution, and also the yeast fermentation test, but he decidedly preferred the first, on account of its superior delicacy.

By modifying the above experiment, so as to test the diastatic changes at varying intervals of time, Paschutin found that starch alone (IV.), subjected to the temperature of the body, will change into sugar spontaneously, but only after considerable time, rarely till after the lapse of a day. The normal infusion itself contains a feeble trace of sugar, which soon disappears on being warmed at 35°–40° C. On the other hand, the mixture of the normal infusion with starch (II.) gives a decided sugar reaction, which increases in intensity for five hours, and then gradually yields to an acid fermentation and the appearance of a precipitate of albumen. The phenomena in bottle I., wherein the infusion had been previously subjected to a high temperature, correspond to the appearances in bottle III., which contains merely the infusion and water, thereby proving that a high temperature is destructive to the ferment of intestinal juice.

From this testimony, it seems indubitable that sugar is formed at the expense of starch under the influence of an infusion of the intestinal mucous membrane, and in the above case was not due to any idiopathic changes, so to speak, in the fluids themselves, or to the influence of organisms introduced from the air. Paschutin adds, that the substitution of the natural secretion for the infusion causes no essential modification of the phenomena, and in comparison, he says, the ferment of the intestinal juice is about equal in strength to a weak solution of saliva. He observed that the intensity of the sugar reaction under the influence of one and the same diastatic fluid depended upon the concentration of the starch paste, upon its relative amount, and particularly upon the temperature in which the sugar formation occurred, and he attributes the negative results obtained by Thiry, Leube and Quincke to a lack of consideration of such points on their part.

Paschutin tabulates the results of his experiments as follows :—

A. The water infusion of the mucous membrane of the small intes-

tine of a dog, exposed for three or four hours to a temperature of 37° to 40° C., yields a voluminous precipitate of albumen, which remains unchanged up to the beginning of decomposition. Simultaneously with the appearance of the albumen, there occurs a decided diminution of the alkaline reaction of the infusion, which sometimes assumes even an acid reaction. Parallel with these phenomena, there disappears from the fluid that constituent which reduces copper oxide in the freshly prepared specimen. Previous boiling of the infusion does not entirely prevent the occurrence of the above phenomena, as, in place of the precipitate, a slight turbidity occurs. Fibrin of the blood and other albuminous materials are not converted into peptones by the infusion. Fat is not decomposed, but only imperfectly emulsified. All the above remarks apply, also, to the infusions of the colon. The natural intestinal secretion gives negative results as regards the digestion of fats, various albuminoid substances, and especially obscure results in regard to the digestion of fibrin.

B. The infusion of the mucous membrane of the small intestine converts starch into sugar. The infusions of other mucous membranes act in an analogous manner, the only difference being quantitative. Diastatic properties are found in the following mucous membranes, in the order of arrangement: in the membrane of the small intestine, urinary bladder, gall bladder, cæcum, colon, rectum and stomach. The œsophagus yields a slimy infusion which has no diastatic power. The secretion from Thiry's fistula in the small intestine changes starch into sugar. No secretion has yet been obtained from a fistula in the colon.

C. Among the infusions of the above mucous membranes, only that of the small intestine (from the pylorus to the valvula Bauhini) contains a ferment which changes cane sugar into grape sugar. This peculiarity, however, is not common to all animals; it was observed in the dog, cat, mouse and rabbit, but not in the ruminantia (he only examined the sheep and calf). The same is true of the natural juice of the intestines.

D. Those infusions which act as well upon starch as upon cane sugar do not contain one ferment possessed of this double virtue, but two distinct ferments, viz., (*a*) the starch ferment, and (*b*) the cane sugar ferment. These ferments may be obtained separately by the following process: After washing the small intestine of a dog and removing the outer layer of muscle, fill it with water under a pressure of a column of water two metres high. The fluid (I.) which filters through under this pressure contains a rich amount of ferment *(a)*, with scarcely a trace of *(b)*. The residue (II.) remaining in the intestine contains ferment *(b)*. To relieve fluid I. of all traces of *(b)*, it should be subjected to a temperature of 37°–40° Centigrade for an hour. The albumen precipitate thus formed carries down with it all of ferment *(b)*; or, again, fluid I. may be refiltered after thoroughly washing out the intestine. If, in the second filtering, too great pressure be avoided and the fluid be allowed to pass through the membranes drop by drop, the filtrate will not contain the smallest trace of *(b)*. Fluid II. may be freed from all traces of *(a)* by further addition of water and filtering.

The infusion of the mucous membrane may be similarly filtered through a previously washed intestine, but as it is rich in albumen, it

requires more pressure, which is a great obstacle to the complete separation of the ferments.

E. A temperature above 40° C. acts destructively upon a solution of an animal ferment which changes starch into grape sugar. A temperature not exceeding 40° C. is entirely harmless to the same. This destructive action of the temperature is modified in its effects by the following conditions, viz., by the degree of the temperature, the duration of its influence and the concentration of the ferment solution. The higher the temperature, the longer its action; and the diluter the ferment solution, so much the more is the damage.

F. If starch paste be exposed to the air, there appears in it a something which will reduce copper oxide. The presence of this reducing agent can sometimes be detected after the lapse of a few hours, but ordinarily not till after two or three days.

These results of Paschutin are practically confirmed by Bernard, who, if not always correct, is always bold in his opinions. As recently as 1856, Bernard maintained that none of the tissues or secretions of the body contained a ferment in the fresh condition. Only in proportion as they had undergone decomposition, could they be said to act as ferments; and hence those secretions which were the most active were said to be the first to yield to decomposition. With equal force and enthusiasm, he now declares that the intestinal juice contributes exclusively to the digestion of the hydro-carbons, and particularly of cane sugar. Its action upon cane sugar, he says, is due to an albuminoid ferment, which it contains, and which he calls "*ferment inversif.*" Amylaceous bodies are digested, he says, very feebly; and of the albuminoid group, fibrin is the only one attacked.

This brings me to the consideration of a number of experiments of my own, which were completed before I knew anything about the results obtained by other men, so that my observations and conclusions, so far as they go, are entirely unprejudiced. I employed Thiry's method of establishing a fistula.

The dog which was associated with me in these experiments was a small mongrel female, in whom the bull-dog element seemed predominant. She was fastened upon her back, and etherized, and then operated on for the fistula. No food was allowed her for a few days subsequent to the operation. Then she was restricted to bread and milk for a few days longer, and finally returned to a regular dog diet. At first, she showed considerable general disturbance, and became quite emaciated; but she gradually regained her flesh and spirits until, in a few weeks, she was as plump and playful as before the operation.

This was in November of 1872; but no digestive experiments were undertaken until the following February, when the external meatus of the fistula was found sufficiently large to admit a medium-sized elastic catheter. I cut off about four inches of the end of a rubber catheter, punched numerous eyelets in it, and then attached it to a short glass tube, which tube was passed through a piece of cork. The cork served two purposes. It prevented the catheter from slipping into the fistula and supported a large test tube for the collection of the secretion which might flow out from the fistula. A slit was cut in the side of the cork to allow the escape of air from the tube as the secretion entered, while a string, attached to the cork and passed around the dog's body, supported the whole apparatus in place. To prevent the dog

from roaming, she was stationed under a horizontal pole, from which she was suspended by ropes passed round her hips and shoulders; only the ropes were so adjusted that they allowed her to rest on her feet, and confined her only when she attempted to move.

Like Thiry, I found that the fistula secreted but little unless irritated, though it was occasionally filled with a yellow, butter-like mass of secretion, from which the water had been reäbsorbed. This plug of inspissated *débris* was the source of great inconvenience, as it both prevented the introduction of the catheter, and plugged up the eyelets, so that no secretion could flow out. By first introducing a glass probe, I sometimes excited sufficient secretion to lubricate the mass, which could then be expelled by external manipulation. Care was necessary in these attempts not to injure the parts, else the integrity of the secretion would be impaired by admixture with blood.

The irritation of the mere presence of the catheter in the fistula was the only stimulant employed for exciting the secretion, and, therefore, I never got a great amount at any one time. The first time the catheter was introduced, I obtained only five drops of the fluid in an hour, which was the smallest amount I ever procured. As the dog and I became better acquainted, she responded more liberally, and once yielded me thirty drops in an hour.

One day, during my experiments, the dog was thoroughly frightened by falling from the table on which she stood, and by my efforts at recapturing her. During the next fifteen minutes, the fistula secreted only one drop, and at the end of half an hour, I was able to collect only three drops. Previously to the fall, she had been secreting at the rate of 14.8 drops per hour. The fact that the digestive processes are so often aborted by mental emotions led me to think that the nervous excitement of the dog consequent upon her fall might have lessened the action of the glands. It is also possible that in the struggle the fistula became misplaced in some way, so that the secretion could not flow as readily as before.

My digesting experiments were conducted as follows: Choosing four glass stoppered bottles, I added to I. twenty-five ccm. of distilled water and ten drops of intestinal juice. Bottle II. received an equal amount of water. Into III., I poured twenty-five ccm. of a hydrochloric acid solution (0.2 per cent.), and ten drops of intestinal juice; and into IV., I poured the same amount of the acid solution without the juice. To each of the bottles I added a few flakes of fibrin and then placed them in a water bath, which was maintained at a temperature of 36°–40° C. by a Bunsen thermo-regulator. To recapitulate:—

Bottle I. contained distilled water, 25 ccm., + fibrin + intestinal juice ten drops.

Bottle II. contained distilled water, 25 ccm., + fibrin.

Bottle III. contained acid solution (0.2 per cent.), + fibrin + intestinal juice 10 drops.

Bottle IV. contained, acid solution (0.2 per cent.), + fibrin.

The fibrin in bottles I., III. and IV. became softened and swollen in much less than an hour, and in the course of twenty-four hours was often entirely broken up, leaving only a turbidity of the fluid, or a slight sediment. As a rule, the fibrin in bottle III. was apparently more affected than in I. or IV. On the other hand, the fibrin in bot-

tle II. appeared to remain unchanged after a delay of a day in the bath.

Having noted the gross appearances, I removed the bottles from the bath and filtered them. Each one was then boiled with nitric acid until all precipitation of albumen ceased, and then they were filtered again. To the final filtrate, I added equal amounts of ammonia. The nitric acid gave a slight yellow tinge to the fluids in I., III. and IV.; and this color was invariably deepened on the addition of the ammonia, proving thereby the presence of peptones. The reaction in IV. compared very nearly to that in I., though they both were exceeded in depth of color by the reaction in III. Bottle II., which contained merely fibrin and water, gave negative results in every instance. These experiments were repeated many times, and always with the same results.

I am aware that my experiments are liable to one objection, in that I did not digest the intestinal juice and water without fibrin to see if the presence of the peptones might not be due to some change in the secretion itself. I think this objection can be met, however, by the record of the gross appearance of the fibrin subjected to only ten drops of the juice, and that soaked merely in water. The former softened, swelled up and dissolved, while the latter was apparently unchanged, even after twenty-four hours. I made repeated tests of solutions of distilled water (25 ccm.) and intestinal juice (10 drops), and also of the acid solution (25 ccm.) and intestinal juice (10 drops). These mixtures, freshly prepared, gave an exceedingly faint yellow color as the result of the test above described; but the color was in no wise comparable to that obtained after the digestion of the fibrin. The fibrin which I employed was obtained by bleeding a dog to death, whipping the blood as it escaped and filtering it through a cloth. It was then washed perfectly clean by running water, and preserved for use in glycerine. On removal from the glycerine, it was squeezed thoroughly, and then washed in distilled water.

On substituting egg for fibrin in the four bottles above mentioned, my results were not so satisfactory. The egg was boiled, and then cut into thin slices through the white part. A few slices were added to each bottle. The egg in bottle II. remained unaffected, even after twenty-one hours, but, on testing the solution for peptones, I obtained, in every experiment, a faint yellow color, showing that peptones were present. This color, however, was eclipsed by that in I., III. and IV. I am unable to explain the presence of peptones in bottle II., unless it be that, during the boiling of the egg, the albumen was affected by some of its own chemical constituents, and that thus the peptones were formed.

Only one experiment was made with regard to the diastatic action of the intestinal juice. A thin, starch paste was prepared, and placed in two bottles (25 ccm. in each). To the first was added fourteen drops of intestinal juice, and then the two were placed in the bath, as in previous experiments. After a short time, all the starch in the controller (II.) had settled to the bottom, leaving the fluids clear above. In bottle I., however, the starch did not form so dense a precipitate, and the fluid was somewhat turbid. On adding Fehling's solution to the controller, and boiling, no sugar reaction appeared,

though the same treatment of bottle I. gave decided evidence of the presence of sugar.

The conclusions which I derive from my experiments, therefore, are, that the intestinal juice converts starch into sugar. Its action upon boiled white of egg is doubtful. Whether alkaline or acidified, it exerts a solvent influence upon fibrin. Thiry declared that the intestinal juice when acidified ceased to act upon fibrin. I found that its action was increased by combining it with a 0.2 per cent. solution of hydrochloric acid. Lastly, I observed that the fibrin was converted into peptones by the action of hydrochloric acid alone.

A *post-mortem* examination of the condition of the dog's abdomen was made on Oct. 10, 1873, which was nearly a year after the operation. The fistula had not been meddled with for several months previous, and was consequently full of the inspissated yellow *débris* before mentioned. This had a peculiar, doggy, musty smell, but no odor of decomposition.

On opening the abdomen, the mesentery was found adherent in every direction to loops of intestine, showing that quite extensive peritonitis had existed. The fistula itself was adherent to a neighboring piece of intestine, and was fourteen centimetres long. It was considerably atrophied, being four centimetres in circumference, while the smallest adjacent portion of the intestine was five and a half centimetres. Near the inner end of the loop, the main intestine was bent at right angles, and held in that position by adhesions. Above the angle, the intestine was distended eleven centimetres in circumference, and contained a hard lump, which proved to be bits of straw, sticks, and other indigestible materials, which could not pass through a slight constriction at that point. Careful inspection of the part detected only a faint trace of a cicatrix to mark the place from which the fistula had been excised. The cicatrix, such as it was, was situated forty-five centimetres above the ileo-cæcal valve, and one hundred and thirty-seven centimetres below the pylorus. On cutting open the intestine and the fistula, the mucous membrane of the latter was found to be of a somewhat deeper, red color, but no microscopical difference could be detected in the epithelium of the two.

Portions of the fistula and intestine were hardened in alcohol, and then carefully compared by microscopical sections. There was, apparently, no structural change in the fistula, except that it was atrophied. Lieberkühn's follicles were distinguishable throughout their entire length, but were much smaller, and more indistinct than they were in the intestine proper. It will be remembered, however, that this examination was made several months after my experiments, and that during those months the fistula had been plugged up with the secretion and undisturbed.

Thus far, I have described the results of observations made upon the small intestines mainly, and but a few words more will be necessary to state all that is known regarding *digestion in the large intestine*.

It was formerly supposed that the cæcum possessed special glands of an acid secretion, and served the function of a second stomach. The acidity of the contents of the different portions and appendages of the large intestine is now known, however, to be due to the acid fermentation of the chyme, inasmuch as the mucous membrane of the parts designated gives a very intense, alkaline reaction.

The secretion of the large intestine has never been collected by Thiry's method, though several attempts have been made. Frerichs, with his loop confined between two ligatures, obtained a fluid similar to that of the small intestine, and greater in amount. Bernard claims that the large intestine is merely the seat of physical and mechanical acts, or of chemical acts which are of no importance as regards the true digestive phenomena. This opinion conflicts somewhat with the observations of Paschutin, who says that separate infusions of the mucous membrane of the cæcum, colon and rectum converted starch into sugar, but did not affect cane sugar, albuminoids or fats.

Kühne quotes the saying that the *processus vermiformis* is good for nothing, except to hurry one to death when any chyme has unwontedly entered it. It is well supplied with Lieberkühn's follicles, however, and, therefore, in some animals, where the process is very long, it must contribute no inconsiderable amount of secretion to the cæcum. Funke tied a string around the upper part of the long appendix cæci of the rabbit without injury to the vessels, and, after two to four hours, examined the secretion. It was slightly ropy, turbid from presence of cylinder epithelium and the products of digestion, and had an intense, alkaline reaction. It changed starch into sugar, and excited acid fermentation, but had no effect upon raw albumen or flesh, either within or without the body.

Funke's results are open to criticism, however, as the fluids which he obtained evidently contained other matters than the pure secretion of the part. Accepting Paschutin's opinions as the most reliable, we may conclude that the secretion of Lieberkühn's follicles in the large intestine differs from that of the small intestine in amount, and in the fact that it does not convert cane into grape sugar. Otherwise, the two secretions are similar in quality.

The following table contains a digest of the experiments thus far described:—

Action of Intestinal Juice on

	ALBUMINOIDS.	STARCH.	FATS.	CANE SUGAR.	GRAPE SUGAR.
Busch.	Protein bodies are dissolved with the appearance of putrescence and the odor of ammonia.	Acts more energetically upon starch than upon protein bodies.	Unaffected; not absorbed unless mixed with bile.	Unaffected.	
Thiry.	Fibrin is crumbled in the normal secretion; this does not occur if the juice be neutralized or acidified. Coagulated white of egg and red muscle are unaffected.	Starch is not converted into sugar.	Unaffected.	No change after hours of digestion.	Intestinal juice has no fermentative influence.
Leube.	Fibrin is crumbled; other albuminoids are unchanged.	Unaffected.		Cane sugar is changed to grape sugar.	Lactic and butyric fermentation occurs as the result of presence of vibriones.
Quincke.	Fibrin is dissolved occasionally; raw or cooked flesh and coagulated white of egg are unaffected.	Starch is changed to sugar after a long time.	Unaffected.		Converts sugar into lactic acid after a long time (12 to 24 hours).
Schiff.	Albumen, fresh casein, cooked or fresh muscle are dissolved in the secretion of the duodenum.	Action on starch almost as energetic as that of pancreatic juice.	Oils are emulsified.		
Wittich.	Fibrin is not digested.	Energetic diastatic action, in one instance.			
Paschutin.	Fibrin and other albuminoids are unaffected by water infusions. Action of normal secretion is especially doubtful on fibrin.	Starch is converted into sugar.	Fats are imperfectly emulsified.	Cane sugar is changed into grape sugar by a ferment found only in the small intestines.	Acid fermentation occurs after a few hours.
Bernard.	Fibrin is the only albuminoid substance attacked.	Acts very feebly on starch.		Chief function of secretion of small intestine is to digest cane sugar; secretion of large intestine is impotent.	
Garland.	Fibrin is crumbled and converted into peptones, whether the secretion be alkaline or acid. Digestion of coagulated white of egg is doubtful Fibrin is digested by a 0·2 per cent. HCl solution.	Starch is changed into sugar.			

It will be seen from the preceding table that nearly all the observers agree that fibrin is digested by the intestinal juice, but that all other albuminoid materials are unaffected by that secretion. Two of them declare that starch is not changed to sugar, while the remainder, with more or less vehemence, assert the opposite opinion. The testimony in regard to fats is entirely negative, except that they are sometimes emulsified.

It seems pretty certain that the secretion of the small intestines converts cane sugar into grape sugar, and Bernard claims that this is the chief function of that fluid. Only four venture to testify to acid fermentation, and, of these, three say that the secretion does produce it.

Fortunately, we have further testimony regarding fermentation in the observations of Planer upon the causes of flatulence. Magendie stated that the gases in the alimentary canal were secreted by the mucous membrane. Planer, however, observed that in normal digestion, gases occurred only exceptionally in the stomach, but were common in the intestines. This led him to a series of experiments, by which he proved that the gases are due to fermentation of the chyme, and that this fermentation is favored by alkaline substances, but is checked by acids. Moreover, the activity of the fermentation grows weaker as its own acid products increase, but is revived by the addition of any neutralizing agent. He concludes, therefore, that the immunity of the stomach from gases is due to the acidity of its secretions which prevents fermentation, while the intestinal secretion favors fermentation by its alkalinity.

He notes a fact of therapeutical interest, namely, that the addition of magnesia to the food increases the formation of gases in the intestines, and particularly of hydrogen gas, while the addition of acid hinders it. Sulphuretted hydrogen gas is found after a flesh diet, but not after vegetables. Oxygen does not occur at all. Hydrogen and nitrogen are common; and coal gas was discovered once in man, but never in dogs.

It may be objected that the amount of digestive energy exhibited in the experiments described is altogether insignificant in comparison with the amount required for the disposal of a good dinner. It must be borne in mind, however, that these are qualitative experiments, merely, and give only the relative action of small amounts of the secretion, when kept in a quiescent condition in a water bath. For a fair quantitative estimate of the value of the intestinal juice, we must await a more definite approximation to the probable amount of the secretion, at the same time remembering, as Dr. Edes says of pepsin, that the results of artificial digestion cannot limit the activity of a secretion in an organ which absorbs the products formed, and which, for all we know, may be capable of rapidly returning to its interior the ferment which still retains its activity.

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